TECHNOLOGY DEPT.

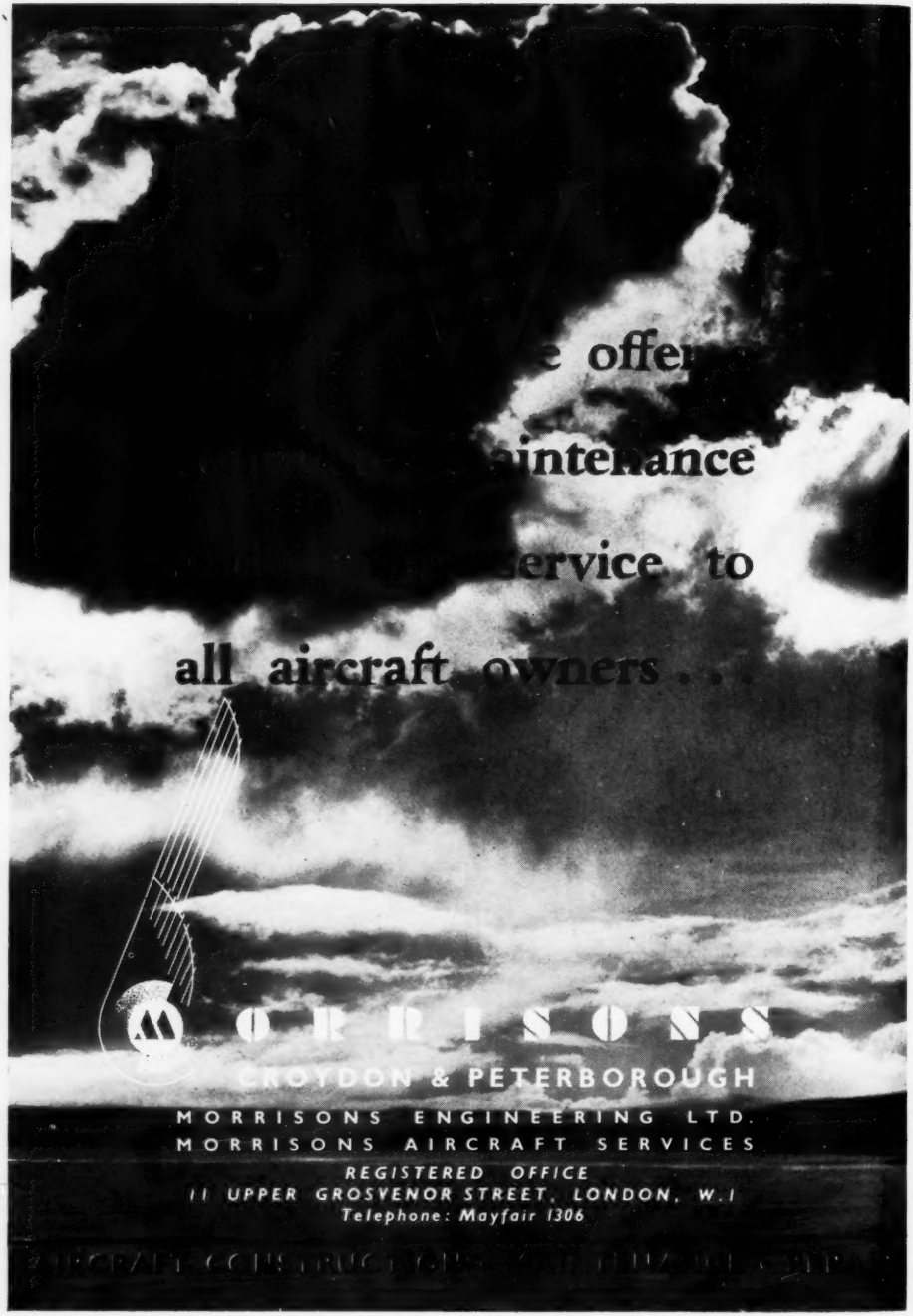
PUBLIC LIBRARY
AUG 21 1946
DETROIT

The Journal of

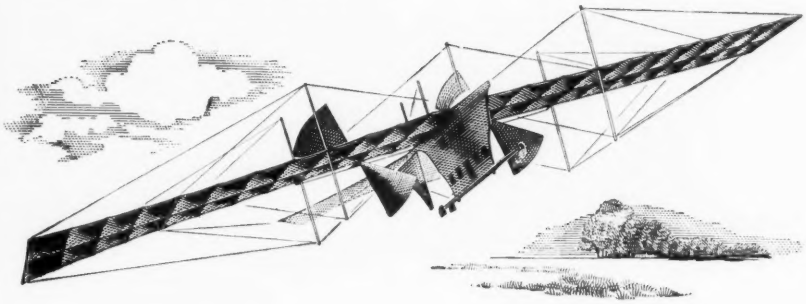
THE ROYAL AERONAUTICAL SOCIETY

AUGUST 1946

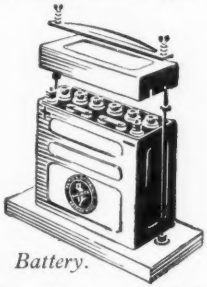
NO. 428 VOL. 50


e offer
aintenance
ervice to
all aircraft owners . . .
MORRISONS
CROYDON & PETERBOROUGH
MORRISONS ENGINEERING LTD.
MORRISONS AIRCRAFT SERVICES
REGISTERED OFFICE
11 UPPER GROSVENOR STREET, LONDON, W.1
Telephone: Mayfair 1306

PIONEERING



AS HENSON, whose 25-h.p. steam-propelled aeroplane of 1843 is illustrated above, ranked among those who pioneered aviation, so the makers of Dagenite batteries pioneered the construction of AIRCRAFT ACCUMULATORS. To-day, because they embody the results of a rich experience, Dagenite batteries are an essential of the most modern aircraft.



Illustrated is the Dagenite Aerobatic Unspillable Battery. Other types for the aircraft itself and for ground starting are supplied.

DAGENITE

AIRCRAFT BATTERIES

PETO AND RADFORD, 50, GROSVENOR GARDENS, LONDON, S.W.1

PR3D/45

D·NAPIER & SON LTD. LONDON. W.3

ELECTRIC ACTUATORS BY

Modern aircraft demand modern controls. The Bristol '170,' Britain's newest commercial aircraft, has Air-shutter controls operated by Ultra Rotary Actuators. With positive 3-position control, the Model R100 gives maximum reliability with minimum maintenance.

Capacity of unit:

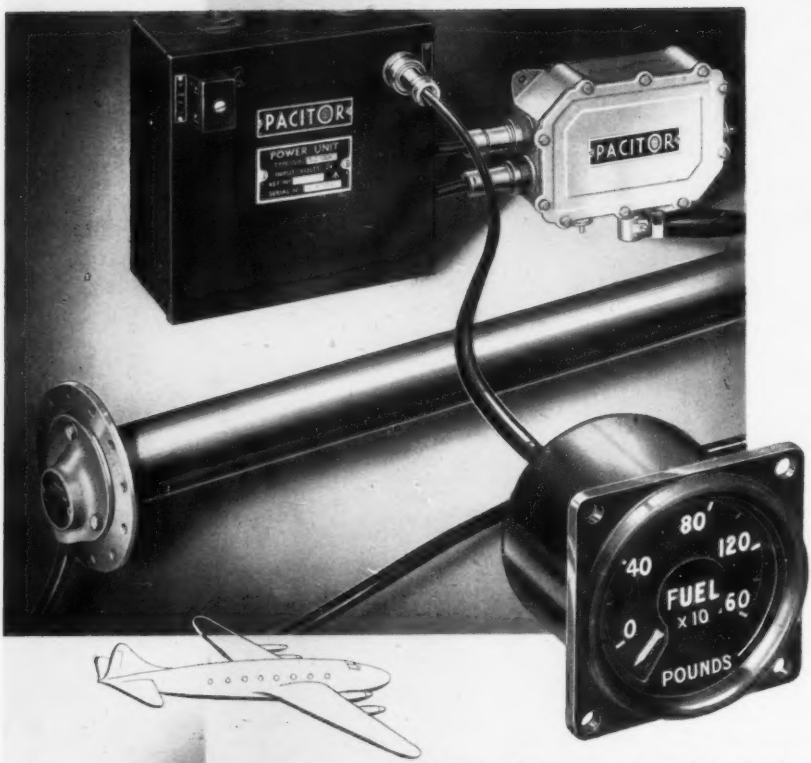
Normal output 300 lbs/in. at 3.2 R.P.M.
Maximum output 500 lbs/in. at 2.6 R.P.M

Also available for:

Fuel valve control
Inter-Cooler Shutter control
Carburettor Pre-heating control
Radiator Shutter control
—*all with 2 or 3 position control*

ULTRA
ELECTRIC LTD
AIRCRAFT DIVISION

WESTERN AVENUE, ACTON, LONDON, W.3



The Pacitor Fuel Contents Gauge, designed and produced by Simmonds Aerocessories Ltd., establishes new standards of accuracy and reliability in fuel measurement. It constitutes the first successful application of the principle of electronics to this vital problem.

Absolute reliability is ensured by the absence of moving parts, and exceptionally accurate fuel measurement by weight or volume can be obtained, regardless of fuel surging, aircraft attitude, or temperature variations. Heavy fuel reserves to safeguard against the inherent deficiencies of mechanical fuel gauges are no longer necessary.

Pacitor Fuel Contents Gauges are fitted as standard equipment on the world's latest types of aircraft.

Pacitor REG'D. *Electronic* FUEL CONTENTS GAUGE

THE WORLD'S MOST ADVANCED FUEL MEASURING SYSTEM

SIMMONDS AEROCESSORIES LIMITED, BRENTFORD, LONDON, ENGLAND

In the making
The "Brabazon" — an unusual aircraft — requires gears of unusual type, manufactured with that exceptional precision and technique associated with the name "David Brown."
Identical facilities are at your service in the development of prototypes and in quantity production.
by DAVID BROWN & SONS (HUDDERSFIELD) LTD

The INSULATOR of the

LODGE

PLUG

is made of

SINTOX

the world's hardest ceramic
(of the same hardness as sapphire)

Nothing but diamond is harder and it will cut glass.

Pink in colour it possesses the highest insulating properties, is a most efficient conductor of heat, and is of astonishing mechanical strength.

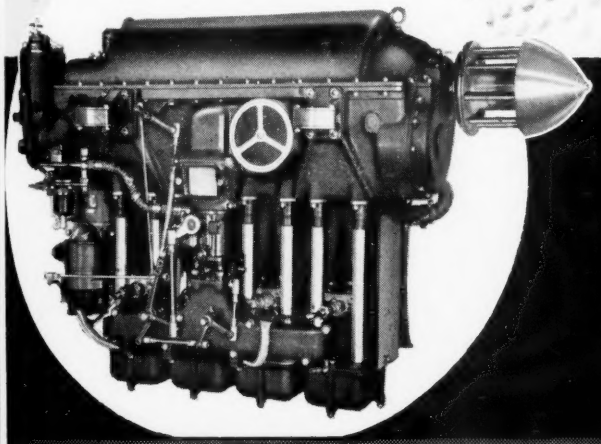
No other make of plug in the world enjoys the unique advantages of "SINTOX" insulation which is exclusive to

LODGE

Lodge Plugs Ltd., Rugby.

A *Cirrus* ENGINE...

powers the "AUSTER" J.1.



The Auster J.1 with Cirrus 'Minor' is a three seater of exceptionally good take-off, climb and landing performances. Loaded to 1,700 lb., it takes off in a 5 m.p.h. breeze in only 150 yds. The landing run is under 100 yds. (stalling speed with full flap 25 m.p.h.). Cruising speed is 100 m.p.h., top speed 120 m.p.h. Grass landing strips of 400 yds. are ample.

"MINOR" Series II	100 h.p.
"MAJOR" Series II	150 h.p.
"MAJOR" Series III	155 h.p.

BLACKBURN AIRCRAFT

BROUGH E. YORKS
Phone: BROUGH 121

AIRCRAFT COURIERS

We are on the threshold of a new era in the transportation of man and merchandise. No form of surface transport can compete for swiftness with the freight or passenger aircraft. Commercial and private operators and owners should avail themselves of the unique experience of Airwork Limited, with its world-wide connections.

THE SERVICES OF AIRWORK LTD:

- Air Transport Contracting
- Operation and management of flying schools and clubs
- Sale and purchase of new and second-hand aircraft
- Fly-yourself and air-taxi hire

IF YOU HAVE AN AIR TRANSPORT PROBLEM CONSULT:

AIRWORK LIMITED, 15, CHESTERFIELD STREET, LONDON, W.1

THE JOURNAL OF
THE ROYAL AERONAUTICAL SOCIETY

With which is incorporated The Institution of Aeronautical Engineers

CONTENTS *AUGUST 1946*

ATOMIC DISINTEGRATION

By Professor N. Feather, Ph.D., F.R.S. :: 553—567

THE ELECTRICAL MEASUREMENT OF STRAIN

By S. C. Redshaw, M.Sc., Ph.D., M.Inst.C.E., F.R.Ae.S. :: :: :: :: :: 568—612

THE EXPANDING DOMAIN OF AEROELASTICITY

By A. R. Collar, M.A., D.Sc., F.R.Ae.S. :: 613—636

BOOK REVIEWS :: :: :: :: 637—638

The Journal of The Royal Aeronautical Society was founded in 1897 in succession to the Annual Reports. For the opinions expressed in Papers that are signed or initialled the authors alone are responsible. None of the papers or paragraphs must be taken as expressing the opinion of the Council of The Royal Aeronautical Society unless such is definitely stated to be the case. **The Journal is published monthly at the Offices of the Society, 4, Hamilton Place, Piccadilly, London, W.1.** Subscriptions per annum, £4 13 0 including postage; Single Numbers 7/6 or 7/9 post free. All communications for publication in the Journal, or on general matters affecting the Society should be addressed to — The Secretary, J. Laurence Pritchard, 4, Hamilton Place, W.1. The Society's Bankers are Messrs. Coutts and Co. All communications respecting Advertisement matters should be addressed to—The Advertisement Manager, Journal of The Royal Aeronautical Society, 4, Hamilton Place, W.1.

Telephone: Grosvenor 3515-3516-3517. Telegraphic Address: Didaskolas, Audley, London.

PAPERS FOR THE JOURNAL

The Council have set aside an annual sum of £250 for the award of premiums for papers published in the Journal. Both members and non-members are invited to contribute original papers on their own special subjects.

The following notes on the preparation of papers for the Journal are given to assist contributors:—

COPYRIGHT

The copyright of every paper printed in the Journal shall be the property of Aerial Science Limited. If the author makes use of copyright material in his paper or information obtained by reason of his employment or otherwise, he must state clearly in his covering letter, or in the paper, that consent has been given for the use of such material.

MANUSCRIPTS

Papers must be in English, in the third person, and typed on one side of the paper only with double spacing. When submitted they must be in their final form for publication. Lengthy additions or alterations after acceptance will not be allowed. Titles of papers should be as brief as possible.

Where practicable a summary of not more than 250 words should be given at the beginning of the paper, giving its scope and conclusions.

All references should be numbered in the text where they occur, and the complete list of references given at the end of the manuscript. These references will not be published as footnotes, but will appear at the end of the text.

ILLUSTRATIONS

Illustrations *must* be drawn so that they will reduce to column or two column width, that is $2\frac{5}{8}$ or $5\frac{1}{2}$ inches. Full page illustrations must reduce to $5\frac{1}{2}$ inches by 8 inches. All drawings must be in black ink on white paper or tracing cloth.

Lettering and figures on drawings *must* be in pencil only. Drawings should be posted flat or rolled.

Photographs should not be less than half plate in size and must be clear black and white glossy prints.

Every drawing and photograph should have on the back its figure number and title.

TABLES

All tables should be kept as concise as possible.

MATHEMATICS

Authors should write in all formulae, except the simplest which can be typed, in their manuscripts. The formulae should be given clearly and sufficient space left for any editorial note for the printer, above and below the formulae. Greek letters should be noted in the margin where first used and the difference between capital and small letters indicated. This is particularly important for Greek letters such as alpha, mu, eta, where confusion may arise with a, m and n. The letter l should *never* be typed is it is always confused with the figure 1.

Unless an abbreviation is one recognised as standard practice, both in this and other English-speaking countries, *e.g.* b.h.p, for brake horse power, the meaning should be given in full on its first use.

The 684th Lecture read before the Society

ATOMIC DISINTEGRATION

by

Professor N. FEATHER, Ph.D., F.R.S.

Professor Feather took the Natural Science Tripos at Cambridge and in 1927 *took up research work under Lord Rutherford. He spent a year at John Hopkins University at Baltimore, returning to Cambridge in* 1930. *In* 1932 *he assisted Sir James Chadwick to obtain cloud chamber confirmation of his discovery of the neutron. In* 1935 *he joined Prof. Chadwick at the University of Liverpool and helped to build up the physics research facilities there, returning to Cambridge in* 1936 *as Lecturer in Physics and Staff Fellow of Trinity College—an appointment he held until* 1945 *when he took up his present appointment as professor of natural philosophy at University of Edinburgh. At the beginning of the war Professor Feather headed a party manning a South-East Coast Radar Station, with the rest of the Cavendish Laboratory staff. Early in* 1940 *he took up work with Bretscher on the measurement of nuclear cross-sections required to establish the feasibility and size of an atomic bomb. He has since been a member of the British Technical Committee which has directed the British group concerned with atomic energy*

A MEETING of the Royal Aeronautical Society was held in the Great Hall of the Institution of Civil Engineers, Great George Street, Westminster, London, S.W.1, on Wednesday, 19th December, 1945, at which a paper by Professor N. Feather, Ph.D., F.R.S., entitled " Atomic Disintegration " was presented and discussed. In the Chair, the President, Sir Frederick Handley Page, C.B.E.

THE idea of the disintegration of the atom appeared first—as a notion seriously entertained by a scientist of repute—in the hypothesis put forward by Rutherford and Soddy in 1903, in an attempt to explain the phenomena of radioactivity as known at that time. The scientist's notion of atoms was then almost exactly a hundred years old, and throughout all but the last years of that century it had sufficed for the purposes of the physicist (though it might not have sufficed for those of the chemist—if the chemist had been a little more bold in his speculative interpretations of phenomena) to consider the atom as characterised merely by mass and size. The concept was of use to the physicist chiefly in relation to the kinetic theory of gases. The atom which he conceived was by nature indivisible, though he could give no logical reason why this should be so. He had no theory, nor any clear views, on the nature of the substance of atoms, and failing such a theory he was always liable to fall back into the obvious error of supposing that the essen-

tial substance of an atom was in some way akin to the solid form of the elementary type of matter of which the atom in question was regarded as ultimate constituent. Clearly any relapse in this direction exposed completely the logical inconsistency of his position.

However, from about 1890 onwards, the more forward-looking among physicists were beginning to move away from this entirely barren point of view. The success of the electromagnetic theory of light had been so complete, and the need for a comprehensive theory of the electrical and magnetic properties of matter in bulk was becoming so urgent, that scientists were being forced to consider " endowing " their atom with an electrical structure. On the other hand, the complexity of the structure which, in their view, was required to take count of the facts still tempered their enthusiasm: the spectrum of the light from an iron arc, for example, consists of thousands of lines—it would have been a bold theorist who was prepared to imagine a structure for the atom of iron (hitherto regarded as structureless!) sufficiently complex to form the basis of a mathematical theory of the multiplicity of separate frequencies which that spectrum reveals. Nevertheless, there were some general phenomena which at first sight appeared sufficiently simple, and Lorentz put forward an explanation of the newly-observed Zeeman effect (the " splitting " of spectral lines when the light-source is subject to a strong magnetic field) which postulated small negatively-charged particles as constituents of all atoms. Also in the self-same year (1897) evidence of quite a different kind—and more direct—was obtained which pointed to the same conclusion. Several physicists—and J. J. Thomson in particular—made observations which indicated that the cathode rays in a discharge tube consist of streams of negatively-charged particles of mass much smaller than that of the lightest atom known—the atom of hydrogen. Thomson continued this investigation during the next two years and proved without the possibility of doubt that identical cathode-ray particles were obtained from all substances when subjected to the discharge—as also in a variety of other ways. Here was the first indisputable evidence for a sub-atomic unit common to the atoms of all types of matter.

Then, from a somewhat different direction, study of the newly-discovered X-rays (Röntgen, 1895) was contributing largely to the reorientation of thought in relation to atoms. The fact (established by Thomson and McClelland in 1896) that irradiation by X-rays renders all gases conductors of electricity was of prime importance in this connection. Experiments of Thomson and Rutherford showed quite clearly that the conductivity could only be understood in terms of ionisation—that is in terms of the production by the X-rays of positively and negatively charged atoms in the irradiated gas. These experiments, moreover, showed—and later work of Rutherford and others confirmed this conclusion—that, in the absence of an electric field removing them to the electrodes or to the walls of the containing vessel, gaseous ions, once formed, persist for times measureable in fractions of a second or of an hour depending upon the conditions.

The initial process of ion-formation soon came to be understood as removal of a cathode-ray particle (negative electron) from an atom which in the normal state is electrically neutral (an atomic positive ion being thereby created as residue)—and, in general, the later attachment of this electron to a second neutral atom to form the corresponding negative ion. Clearly, the idea of the immutable atom had been left behind once for all as soon as the views here outlined were accorded common acceptance, but the transient nature of the phenomena of ionisation (all positive and negative ions eventually recombine if left to themselves for a sufficient length of time) provided the obvious refuge—explicit or implicit according to the circumstances—of those who did not wish their philosophical outlook upon the world to suffer too sudden a change.

It was against this background, then, that

the Rutherford-Soddy hypothesis was put forward in 1903. The radioactivity of uranium and thorium substances had been studied for a full six years and a great variety of novel effects had been observed. The novelty of the effects made it natural that some experimenters should concentrate on one aspect of the matter rather to the exclusion of the rest, but Rutherford and Soddy attempted to provide an all-embracing hypothesis sufficiently wide in scope to bring all established results into a single framework of explanation. We may list some of these results as follows: uranium and thorium emit radiations spontaneously—that is without external stimulus and at a rate which is not affected by any agency available to the physicist; the radiations consist in part of negative electrons projected with high speed (β-particles) and in part of much heavier positively-charged particles (α-particles); in the case of uranium and thorium the emission does not appear to suffer any diminution of intensity with lapse of time; other chemical substances, notably radium, can be separated from uranium minerals which show similar effects, weight for weight with much greater intensity than does uranium itself; these effects also appear to a first approximation constant with time; highly active preparations of radium maintain themselves at a temperature a few degrees higher than that of surrounding matter; radioactive preparations can be separated from uranium—and more especially from thorium—compounds which contain active substances the activity of which decreases with lapse of time.

It was the last result, due primarily to the experimental work which Rutherford and Soddy had themselves carried out, which was regarded by these brilliant pioneers as providing the clue to the whole problem. They were not certain of the chemical nature of several of the short-lived radioactive substances which they separated, and whose radiations they studied, but they were convinced that their preparations contained new elements—though present in such minute amount that they were undetectable but for their radiations. They found one feature common to all. The intensity of the radiation, in every case in which they had a " radioactively pure " preparation, decreased with time according to an exponential law:—

$$I = I_0 e^{-\lambda t}$$

Here I_0 is the initial intensity and I the intensity of the preparation after a time t, and λ is the "disintegration constant." The form of the law was found to be the same for all pure preparations, only the magnitude of λ differed from one to another. Radioactivity had been shown by Becquerel to be an *atomic* property in the case of uranium—unaltered, for example, by the chemical combination of uranium atoms with the atoms of other elements; Rutherford and Soddy regarded the disintegration constants which they determined as the only *atomic* constants then known for the new elementary substances which they believed their preparations to contain. By extrapolation from their experiments they assumed that all pure radioactive substances (including uranium and thorium) would ultimately be found to be decaying in activity continuously, those for which the activity appeared at first sight to be permanent merely decaying at a very slow rate (that is being characterised by a very small value of λ). Their disintegration hypothesis postulated that radioactivity is the spontaneous disintegration of atoms, the disintegration connstant λ representing, for any " radioactively pure " preparation, the fraction of the atoms present likely to disintegrate in the (chosen) unit of time. Disintegration itself they described as the emission of an α-particle or a β-particle from the radioactive atom —leaving as residue an atom of a different chemical species, which might itself also be radioactive. They introduced the notion of disintegration series, explaining their experimental results by saying that the disintegrations of atoms of uranium and thorium were followed by a whole sequence of successive disintegrations in each case in which other

radioactive species of more or less transient existence were produced.

We cannot do more here than say that, in spite of all the scepticism of the chemists, this bold hypothesis was never seriously challenged. We may, however, note certain further features concerning it. In the first place, it describes what is essentially a statistical effect: the " laws of chance " " govern " the whole phenomenon, the disintegration constant gives the most probable fraction of the atoms present which suffer disintegration in unit time—it is in essence a probability constant characterising the type of atom concerned. This view of the matter is obviously consistent with the experimental observation that nothing can be done either to hasten or retard the process of transformation.

Secondly, the disintegration hypothesis, although it renders untenable any belief whatever in the immutability of atoms, or of the chemical elements themselves, allows of the retention of the principle of the conservation of energy, so dear to the physicist. There is nothing inexhaustible about the energy of radioactive transformation (it was soon established that the disintegration series, both of uranium and thorium, terminate in a stable end-product, which in each case has the chemical properties of ordinary lead)—nor any mystery in the generation of heat by highly active preparations of radium: the α- and β-particles lose kinetic energy in passing through matter and, in conformity with the first law of thermodynamics, this energy eventually becomes sensible as heat. Finally, the details of the picture provide yet another instance of the need to ascribe structure to the atom; they would appear to point obviously not only to negative electrons but also to positively-charged heavy particles (α-particles) as structural units. Since any satisfactory theory of structure must involve both positive and negative elements, this requirement is not in itself unreasonable.

During the ten years which followed the publication of the disintegration hypothesis the problem of atomic structure received widespread attention and, so far as a semi-popular account is concerned, it may be said, with but one proviso, that by the end of this period (1913) all the important structural units had finally been recognised. Experimental investigations on the modes of production of X-rays and their interaction with matter had thrown considerable light on the numbers and arrangements of the negative electrons in the atoms of many elements, and a close study of the passage of α-particles through various materials—particularly in relation to the less frequent types of interaction thereby revealed—had led Rutherford to propose the nuclear atom-model which has since been universally accepted. The essence of this model is that the whole of the positive charge in the atom is regarded as concentrated in what is effectively a single particle (nucleus), endowed with practically the whole of the mass of the system. Most of the atom is "empty-space," populated, and with its boundary defined by, the motions of the atomic electrons. This view of atomic structure was made inevitable by the fact that α-particles were found to travel through matter almost without deflection: in doing so they obviously pass completely *through* individual atoms, both the α-particles and the massive parts of the atoms (the nuclei) being so small (in comparison with the size of the atom itself) that they very rarely come into effective collision. We may make the rather inexact statement that such collisions occur not more frequently than once for every hundred million atoms traversed, and conclude (with an equal disregard of rigour!) that the diameter of the nucleus is of the order of one ten-thousandth of the diameter of the atom as a whole.

The quantitative evidence from experiments on X-rays and that from the altogether different experiments on α-particle scattering was shown, on this view, to be capable of complete correlation in a single scheme. In order to give this scheme numerical precision it merely had to be assumed that the number

of " extranuclear " electrons in the atom of any chemical element (or the number of unit positive charges on its nucleus, which is the same number) was identical with the ordinal number appropriate to the position occupied by that element in the natural sequence of the periodic table. Here was a fusing of concepts drawn from diverse branches of science of such utter simplicity that it could not fail to be recognised as essentially correct. No developments which have since taken place have, in fact, shaken the scientist's belief in its fundamental significance.

We have said, on the other hand, that there is one proviso to be made. In the light of the nuclear hypothesis we need to restate our views on the mechanism of radioactive disintegration and make them more precise. Radioactive disintegration involves, in each case, a change in the chemical nature of the substance concerned; in many cases, as we have said, it also involves the spontaneous emission of heavy positively-charged particles from the atoms which disintegrate. It is clear, from a consideration of these facts, that radioactivity is a property of the nuclei of atoms rather than of the atoms as a whole: a change of nuclear charge is necessary if the chemical nature of the substance is to change —and the nucleus is the only sub-atomic structural unit which is at once both positively charged and " heavy." But, if the nuclei of atoms are subject to disintegration, throwing out in the process fragments of their substance (α- and β-particles), then obviously it is necessary to think of atomic nuclei as themselves possessing structure. This is the proviso which was to be made. The nuclear atom of the Rutherford-Bohr hypotheses of 1911-13 presents a complete picture of the atomic structural pattern, except in so far as it does not specify at all what are the common structural units or what is the general pattern of structure of the nuclei themselves. This defect was trivial in relation to most of the earlier applications of the new hypothesis, but obviously it is not so when radioactivity or any similar phenomenon is in question. And it is, of course, such phenomena which are our primary concern.

Three dates within the past thirty years mark the turning points in the development of the whole subject of the physics of the nucleus: 1919, 1932, and 1939. Nuclear disintegration which was not spontaneous (artificial disintegration) was first observed, by Rutherford, in 1919; the neutron was discovered, by Chadwick, in 1932—and in the same year artificially accelerated particles were first used to replace the naturally emitted α-particles as disintegration-producing projectiles; in 1939 the occurrence of neutron-induced fission of uranium was established, by Hahn and Strassmann on the basis of chemical, and by Meitner and Frisch on the basis of physical evidence. We shall notice each of these developments in turn.

Rutherford's observation in 1919 was to the effect that, amongst the already rare encounters between α-particles and the nuclei of the atoms of matter through which they pass, there may be a few—in the case of nitrogen, at least, say one in a thousand—which result in the liberation of another particle from the nucleus which is struck. During the next few years several examples of this type of transformation were found with the lighter elements, and experiments by Rutherford and Chadwick showed that the new particle which is ejected is generally a proton —that is the nucleus of an atom of hydrogen. The α-particle had already been shown to be itself the nucleus of a helium atom, and since later work proved conclusively that in these disintegration collisions the impinging α-particle penetrates into and is " captured " by the heavier nucleus, we see that in these observations clear examples of transmutation had been recognised. We may represent the nitrogen disintegration as follows:—

$$^{14}_{7}N + ^{4}_{2}He \rightarrow ^{17}_{8}O + ^{1}_{1}H$$

The colliding particles are represented on the left, with superscripts and subscripts indicating the mass and charge numbers which are

appropriate*, and the resultant particles on the right. Because nuclear charge must be conserved, and because the charge on the α-particle (helium nucleus) is one unit greater than that on the proton (hydrogen nucleus), the heavier product nucleus must have one unit more charge than the " initial " nucleus of nitrogen which was struck. The product nucleus is, therefore, the nucleus of an atom of oxygen (of mass number 17). We shall return to the general question of nuclear mass at a later stage, meanwhile two important results of observation should be recorded.

Rutherford and Chadwick found that, if they used α-particles of one single kinetic energy for their experiments, the protons which were obtained in the transmutation process differed in energy from one element to another. They found that in some cases, as with nitrogen, the protons never had as much energy as the α-particles which caused the disintegrations, but that there were cases, as with aluminium, in which some of the ejected protons had more energy than the α-particles which were used. Secondly, they found that the transmutations became more and more rare as the atomic weight of the element which they bombarded was increased. The first observation demonstrated without question that nuclear transformations may be brought about which result, in balance, in a release of energy (as the " exothermic " chemical reactions involved in combustion do), the second was explained on the basis of the electrical repulsion which the positively-charged α-particle experiences in its approach to any other (positively-charged) nucleus: as the nuclear charge increases a stage is reached when this repulsion effectively prevents the close collision which is necessary if the two particles are to interact in a way that leads to disintegration. Only concerning the first observation should we add this further remark. As we have said, the disintegrations observed by Rutherford and Chadwick were essentially rare events: if it required the " firing " of say, a million and one α-particles into a sheet of aluminium in order to register one successful " hit " on an aluminium nucleus, it was at that stage of no more than academic interest to know that in the one favourable collision useful energy was set free. The energy of the other million α-particles was lost so far as useful work was concerned.

It has been said that in general the disintegration particles observed by Rutherford and Chadwick were protons. In 1932 certain effects which had been found with beryllium, when bombarded by α-particles, were reinvestigated by Chadwick. Chadwick advanced the view that these effects could much more reasonably be explained as evidence for the liberation of uncharged particles (neutrons) than in any other way. The suggestion was greatly strengthened by more exact measurements and has since been unchallenged, the nuclear transformation in this case being written—

$$^{9}_{4}Be + ^{4}_{2}He \rightarrow ^{12}_{6}C + ^{1}_{0}n$$

The discovery of the neutron, for such it was, had two important consequences, the one theoretical, the other a practical one. The theoretical advance which the discovery made possible was that it permitted, for the first time, a rudimentary theory of nuclear structure to be developed. The neutron is an uncharged particle of mass very little different from the mass of the proton. Both protons and neutrons, in certain circumstances, can be liberated from nuclei. It was natural that the simple hypothesis that neutrons and protons constitute the fundamental building units of nuclei should be investigated in detail—and it was soon found that this hypothesis could be developed in such a way as to represent all the experimental facts which were known. The experiments of Aston and (later) those of Bainbridge and others had shown that though the chemical elements are frequently mixtures of isotopes (isotopes are

* " Charge number " is used synonymously with " atomic number " to indicate the number of unit positive charges on the nucleus *(v. sup.)*; mass number is the physicist's equivalent to the " atomic weight " of the chemist *(v. inf.)*.

atomic species for which the nuclear charge is the same, but the atomic mass is different, from one to another), yet the masses of all atoms are very nearly integral multiples of one-sixteenth of the mass of the commonest oxygen isotope $\left(\begin{smallmatrix}16\\8\end{smallmatrix}O\right)$ taken as unit (the atomic weights of the chemist are *not* always integral in terms of $O=16$). The "mass number" of a nucleus, to which we have already had reason to refer, is a definite quantity for just this reason: it is the integer from which the "exact" atomic mass (in terms of $\begin{smallmatrix}16\\8\end{smallmatrix}O=16.000$) differs by a small fraction of a unit at the most. Let us represent it by A—and let the charge number, which we have already defined, be represented by Z. Then the neutron-proton hypothesis states that the nucleus in question (the nucleus $\left(\begin{smallmatrix}A\\Z\end{smallmatrix}\right)$, we may say) consists of an intimate union of Z protons and $(A\text{-}Z)$ neutrons. The α-particle, on this view, consists of two protons and two neutrons in close union, the nucleus of the common isotope of uranium of 92 protons and 146 neutrons. One significant feature of this analysis is that it appears that in every case, without exception, the "exact" mass of a nucleus is somewhat less than the sum of the "exact" masses of the Z protons and $(A\text{-}Z)$ neutrons which are assumed to make up its structure—less by some few hundredths of a mass unit for the lightest nuclei, and with the "mass defect" in question steadily increasing to some two mass units for the heaviest nuclei known. We shall return to a further consideration of this very significant feature later*.

* The process of emission of a β-particle (negative electron) from a nucleus might appear at first sight to be less satisfactorily covered by the neutron-proton hypothesis. The problem cannot be entered into here, but it should be said that the current view is that the electron escapes in a type of "internal" transformation in which a nuclear neutron simultaneously changes into a proton (which remains in the nucleus).

The second important consequence of the discovery of the neutron—the practical consequence—was that it provided a new projectile with which to bombard other nuclei. We have mentioned the disadvantage that the α-particle is itself positively charged, and therefore cannot approach very close to a heavy nucleus unless it is projected with enormous kinetic energy. The neutron, being uncharged, suffers from no similar disability Any given source of α-particles (from radium and its products), when mixed with beryllium, liberates neutrons to the extent of a little more than one neutron per ten thousand α-particles fired into the beryllium, but this loss of intensity is more than compensated for by other factors, in particular the lack of charge already referred to. Disintegrations produced by neutrons in nitrogen—

$$\begin{smallmatrix}14\\7\end{smallmatrix}N+\begin{smallmatrix}1\\0\end{smallmatrix}n\rightarrow\begin{smallmatrix}11\\5\end{smallmatrix}B+\begin{smallmatrix}4\\2\end{smallmatrix}He$$

—were observed within a few days of the discovery of the neutron itself; disintegrations of practically every element up to the heaviest (uranium) had been reported within two years of that discovery.

In summarising the important stages in the development of the science of nuclear physics it was said that the year 1932, in addition to the discovery of the neutron (and neutron-induced transformations), also saw the utilisation of artificially accelerated particles for the first time as nuclear projectiles. This came as the result of a great burst of activity in specialised electrical engineering directed towards the attainment of high voltages either by direct or by indirect means. It will be appreciated that if a hydrogen atom is ionised (as in a discharge tube), since in the neutral state the atom contains only a single electron, in the ionised state the positive ion is itself a bare nucleus—a proton. If, therefore, this ion is accelerated by the application of an intense electric field it will issue from that field with all the properties of a proton of high energy—with properties comparable in fact with those of protons ejected from nuclei under α-particle bombardment. More-

over, many more high energy protons can be obtained in this way by acceleration than can be produced using α-particles to bring about nuclear transformation. In principle one might have used " disintegration protons " for the further bombardment of nuclei in the way in which " disintegration neutrons " are employed, but this was not practicable for reasons which should be obvious (the proton, unlike the neutron, suffers from the disability of positive charge, as does the α-particle); the development of means of acceleration, however, provided an entirely practicable alternative method of employing these particles as projectiles. The first success was obtained by Cockcroft and Walton in Cambridge using a device which maintained a constant potential difference of 600,000 volts across the accelerating tube (the protons which are liberated in nuclear transmutations have kinetic energies which, in general, correspond to acceleration through potential differences of from one to ten million volts).

Their initial result was spectacular: on bombarding a target containing lithium with these relatively low-energy protons they obtained clear evidence of transmutation—and they were able to show that the sole product of transmutation in this case was helium, two α-particles from each successful proton " hit," with the α-particles carrying away about thirty times as much energy as the proton brought to the collision! We write, in representation of the qualitative result,

$$^{7}_{3}Li + ^{1}_{1}H \rightarrow ^{4}_{2}He + ^{4}_{2}He$$

Here, of course, the useful bonus of energy per disintegration is many times greater than that obtained in the case of the α-particle bombardment of aluminium, already noted, but on balance the position is no more favourable—a multiplication factor of thirty for each proton which makes a successful hit, but possibly a thousand million protons " wasted " for every one which produces transmutation. Naturally, an enormous impetus was given to further research by the publication of the results obtained by Cockcroft and Walton, and with other methods of acceleration of particles rapidly reaching the operating stage, progress was assured. Furthermore, a rare isotope of hydrogen had just been discovered (" heavy hydrogen " or deuterium, of mass number 2): very soon it was possible to employ accelerated streams of deuterons (heavy hydrogen nuclei) as well as fast protons in the new machines. In this line of work the cyclotron of Lawrence and the electrostatic generator of van de Graaff quickly established themselves as experimental tools of great power and flexibility.

One or two results of the early work should certainly be mentioned here. First, we may notice a case of transmutation by deuterons in which the release of energy is even greater than it is in the proton disintegration of lithium referred to above. When deuterons are used with the same target material, atoms of the lighter isotope of lithium are involved in the transformation—

$$^{6}_{3}Li + ^{2}_{1}H \rightarrow ^{4}_{2}He + ^{4}_{2}He,$$

and in this case energy is set free corresponding to 22 million electron volts, compared with 17 million electron volts in the previous case. Secondly, and of considerable practical importance, it was found that neutrons are produced in many disintegrations brought about by proton and deuteron bombardment. Thus neutrons can be obtained for experimental purposes without the use of strong sources of radioactive materials, itself a great convenience. We may indicate some of the transformations commonly used to this end:

$$^{2}_{1}H + ^{2}_{1}H \rightarrow ^{3}_{2}He + ^{1}_{0}n$$

$$^{7}_{3}Li + ^{1}_{1}H \rightarrow ^{7}_{4}Be + ^{1}_{0}n$$

$$^{7}_{3}Li + ^{2}_{1}H \rightarrow 2\,^{4}_{2}He + ^{1}_{0}n$$

$$^{9}_{4}Be + ^{2}_{1}H \rightarrow ^{10}_{5}B + ^{1}_{0}n$$

It is clear from this incomplete list that neutron sources in great variety are now avail-

able to the experimenter. Finally, we mention the fact that (it seems for an interesting, but rather recondite reason) the positive charge on the deuteron is not so prohibitive of interactions between deuterons and the heaviest nuclei as might be imagined from what has already been said. Thus transmutations under deuteron bombardment have been observed for almost all elements, up to and including thorium and uranium. With gold, for example, an isotope of mercury is obtained—inverse alchemy, though involving minute amounts only!

At this point, perhaps, we should take up more formally the question of mass and energy before proceeding to the later developments, the discovery of nuclear fission and the possibilities in relation to "atomic" power which derive from it. It has been said that the disintegration hypothesis of Rutherford and Soddy salvaged the conservation laws by the emphasis which it placed on the transient nature of radioactive effects of all kinds. Even the liberation of energy by uranium and thorium was, in the light of this hypothesis, seen as an exhaustible phenomenon. But, in truth, that is too facile and optimistic a view of the matter, until it has also been shown what are the stores of energy which are effectively exhausted in the finite process. The physicist must balance even the limited account in all cases. The later work on artificially produced transformations poses similar questions. What is the store of energy which is tapped in the α-particle disintegration of aluminium or the proton-induced disintegration of lithium? The answer may now be given—and given in two stages, which may be referred to briefly as representing the qualitative and quantitative aspects of the problem. Since we have attributed all the effects with which we are here concerned to the nuclei of atoms, we may obviously say, qualitatively, that the energy which is released is the energy of constitution of the nuclei involved in the transformations. We have become accustomed to adopt the corresponding viewpoint in relation to the energy released in chemical processes—say the heat liberated in combustion—here we merely propose the extension of an accepted point of view to cover a new range of phenomena. If the products of a chemical reaction possess less intrinsic energy than the reactants, then the balance is set free when reaction occurs (if the reverse is true then heat must be supplied continuously if the reaction is to proceed). Quantitatively, even, the problem may be pursued, in the chemical case, without introducing any fundamentally new idea: it should be possible in principle to calculate the amount of intrinsic energy available, in terms of the changes in the electron configurations which take place when the atoms in question react in the way considered.

When we pass to the quantitative stage in the nuclear case, however, we encounter a different situation. We have no completely satisfactory theory of nuclear constitution; certainly no theory which will permit the prediction of values of the intrinsic energy to the degree of precision required in our problem. But we have, in the background of the general physical picture, one of the basic tenets of the theory of relativity: the present-day relativist holds that mass is latent energy, that is, he believes that what the ordinary man regards as the substance of things is merely another manifestation—albeit an essentially static manifestation, so far as our gross senses are concerned—of that attribute of systems to which the physicist has given the name of energy. Quantitatively, he says, the amount of energy (E) latent in a mass m is given by the equation

$$E = mc^2$$

where c is the velocity of light. This point of view was current amongst mathematical physicists for a quarter of a century before it was put to the direct test of experiment; once it has been enunciated it is not surprising that it was in the realm of nuclear physics that the most exacting experimental tests ultimately proved possible. Here, for the light atoms at any rate, highly accurate values of the "exact" masses of nuclei may be derived

from measurements with the mass spectrograph (Aston, Bainbridge and others). Thus the masses of the proton, the α-particle and the lithium nucleus $\left({}^{7}_{3}Li\right)$ were known at the time that the disintegration experiments were carried out, and it soon became evident that the energy which appeared as energy of motion of the α-particles could be exactly equated (on the basis of the relativity formula) to the loss in mass-energy in the transformation (the small amount of kinetic energy contributed by the incident proton being allowed for). We may draw up a balance-sheet for this process, using the latest values of nuclear masses, as follows:—

Initial masses,		Final masses.	
${}^{7}_{3}Li$	7.01654	${}^{4}_{2}He$	4.00277
${}^{1}_{1}H$	1.00758	${}^{4}_{2}He$	4.00277
	8.02412		8.00554

Mass lost: 0.01858 mass unit.

Energy gained 17.06 million electron volts.

As we have already stated, when the appropriate change of units is made (on the basis of the relativity formula 0.001 mass unit corresponds to 0.931 million electron volts), these two quantities are identical within the limits of experimental uncertainty. The same result has been obtained whenever the production or absorption of energy in nuclear processes has been subject to exact investigation: energy released involves disappearance of mass, energy absorbed in the nucleus of an atom involves the " creation " of additional mass—and the relativity formula applies exactly to these transformations.

We should not, however, restrict our statement of the matter by limiting the consideration to nuclear changes only; the relativist would insist that the general result applies equally to all changes. Thus, in spite of the exact experiments of the chemists, he would insist that *some* mass disappears in combustion—though the two authorities would immediately reconcile any apparent difference of opinion once quantities were stated. The relativist merely requires a disappearance of mass, in this case, of the order of one part in some fifty thousand million—which the most scrupulous gravimetric chemist would certainly never miss!

Returning to the nuclear domain there is one further point to be mentioned. We have seen that the " exact " mass of any nucleus is always less than the sum of the masses of the neutrons and protons of which it is currently supposed to be constituted. This is an additional point in favour of the accepted view: we suppose that in the building up of the nucleus, at some time in the distant past, mass was transformed into energy and "lost." The nucleus in question does not now break up into its constituent particles spontaneously just because the lost energy needs to be supplied before such dissociation can take place. Under present conditions this energy is not naturally available.

With this background of theory we are now in a position to discuss the most recent discoveries. In all the nuclear transformations with which we have been dealing hitherto, we have been concerned at the most with changes in nuclear charge of two units and in nuclear mass of four units (confining attention to artificially produced transformations, changes of nuclear mass of more than three units have not so far been encountered). There is a general reason for this limitation: it is much the same as applies to the entry of charged particles into the nucleus. The higher the charge the more difficult it is for a positively-charged particle to emerge from a nucleus and escape, unless the energy available is itself very high. This general principle was so firmly fixed in the physicist's mind that it delayed for a couple of years any clear appreciation of certain peculiar effects observed with uranium and thorium during the period 1935-39. Then, early in the latter year, incontrovertible evidence appeared which clinched the question of interpretation once for all. Hahn and Strassmann found that among the

products of the neutron bombardment of uranium there occurred "artificially radioactive" isotopes of barium and lanthanum—and very quickly a large number of other active species of "intermediate" mass and charge were identified as products of the same bombardment.* Now the nuclear charge number of uranium is 92, that of barium 56 and of lanthanum 57. Isotopes of bromine (35) and krypton (36) were amongst the other products. It was clear that profound changes in nuclear charge and mass were taking place. From the physical side, Frisch detected the individual nuclei at the moment of their formation, and it became immediately obvious that these nuclei were being produced with extremely high energies of motion. All the requirements for a revision of the previously cautious attitude of the physicist were thereby provided, and the effects which had for long been so puzzling were recognised as due to the "fission" of the uranium nucleus, that is to its division into two roughly equal fragments after capture of a neutron. In order to explain the great variety of fission products obtained, it was merely necessary to assume that the process of division could take place in a number of different ways with almost equal probability. We may write as representing one of these modes of division

$$^{238}_{92}U + ^{1}_{0}n \rightarrow ^{145}_{56}Ba + ^{94}_{36}Kr$$

Naturally the discovery of nuclear fission was an event of the utmost interest to physicists, and the outburst of (often rather hastily conducted) researches on this subject can be paralleled only by the widespread experimental activity among men of all classes which followed Röntgen's announcement of the discovery of X-rays in 1895. In the case of fission, however, there were fewer self-confessed amateurs in the field, and within a period of months the essential features of the process were already fairly well understood. It was soon established that, under neutron bombardment, fission occurred with the nuclei

$$^{230}_{90}Th,\ ^{232}_{90}Th,\ ^{231}_{91}Pa,\ ^{235}_{92}U,\ \text{and}\ ^{238}_{92}U$$

—that is with all the isotopes of the elements of atomic number greater than 89 which are available for experiment in sufficient amount (isotopes of thorium (90) protactinium (91) and uranium (92)). And later, fission under bombardment with protons, deuterons, α-particles and γ-rays was also observed. A semi-empirical theory of the process was developed (Bohr and Wheeler) in which the basic idea of "instability of form" was the central notion—and cogent reasons were advanced why the onset of this particular type of instability should be relatively sudden as the nuclear charge is increased. For the first time ideas, based directly on experimental evidence, were generally accepted which permitted an explanation of the limitation of the number of the chemical elements to ninety-two—or thereabouts. It was realised that if the nuclear charge were substantially increased beyond this amount (92 units of positive charge—as in the case of uranium) nuclei would be obtained which suffered fission spontaneously.

These were important results, but it will be recognised that they are of academic rather than of practical interest. The emergence of practical possibilities in relation to nuclear energy came with the discovery (by Halban, Joliot and Kowarski, and later by others independently) that, in addition to the fragment nuclei of intermediate mass which we have already discussed, "secondary" neutrons are also liberated as a result of fission. The original fragments such as $^{145}_{56}Ba$ and $^{94}_{36}Kr$ (*v. sup.*) are unstable in the sense of being over-heavy for the charge carried (the heaviest stable isotopes of barium and kryp-

* Space is not available in this article for a discussion of the general question of "artificial" radioactivity. Suffice to say here that the products of nuclear transformation are, more frequently than not, unstable (radioactive) isotopes of the common elements which subsequently transform into stable species by the emission of electrons (β-particles and positive electrons).

ton have mass numbers 138 and 86 respectively), and one way in which the balance may to some extent be restored is quite clearly by the emission of neutrons from the fragment nuclei first formed. Whatever is the precise sequence of nuclear events does not, however, matter for our purpose; direct experiment shows that on the average two or three secondary neutrons are liberated per fission. This is the all-important conclusion: if a mass of uranium is taken which is large enough and pure enough it should be possible to utilise these secondary neutrons for the production of further fissions—the nuclear reaction should become self-propagating (as combustion is self-propagating under suitable conditions). It is common knowledge that the successful realisation of the conditions for the self-propagating (divergent) nuclear reaction have been achieved by the scientists working in the United States on the atomic bomb project; it is only necessary to add here that this achievement may take either of two forms. The system may be so arranged that the divergent reaction is uncontrolled—the bomb at the moment of fusing is such a system—or it may be arranged in such a way that continuous regulation is possible. Happily the details of the fission process are such that this, also, is an entirely practicable proposition, and uranium " piles " have been in steady operation in America for periods of months at a time. When the nuclear reaction is controlled in this way very large amounts of energy are liberated as heat, new chemical species are formed and artificially radioactive bodies are produced, as fission products, in great profusion.

The development of means of utilising the heat industrially, the study of the new elements and the application of the radioactive substances in the treatment of disease will, without doubt, represent major lines of advance in industrial power production, in pure physics and in the arts of medicine in the years which lie ahead. It is not, however, part of our purpose here to prophesy in detail where such advances may lead.

DISCUSSION

MR. W. S. FARREN, C.B., M.B.E., M.A. (Fellow): He had known Professor Feather for a long time, but had never expected to hear him dilate on the lack of courage of physicists or on their lack of foresight, and then, in his concluding sentence, hand the baby to the engineers. No one nowadays was unaware of what a dangerous place the physicists had made of the world. Whether they owed it to their own courage and foresight, or to the help of engineers, or a mixture of the two, he did not know. But it did not seem to him that there was any need to accuse them of lack of initiative. In the future application of their discoveries, engineers would have to help them to confine and transform the excessive heat they had produced into more convenient forms.

Some of the things which Professor Feather had said would have made a deep impression on all who had heard him. One in particular was his emphasis on how much the chain of discoveries—perhaps the most important discoveries that had ever been made—owed to the devising of outstandingly simple methods, or of ways of simplifying things, which, although known previously, had been too complex for most to understand. It was not often that the members of the Royal Aeronautical Society had the opportunity to think about the fundamentals of the world. As engineers, they were mostly concerned with complicated problems of their own creation.

A thought which occurred to him when listening to Professor Feather's lecture was that great men came along at the right time. Professor Feather had his place among those people. He would like to express to him, on behalf of the meeting, their grateful thanks for having given them such an opportunity to think again.

MR. B. N. WALLIS, C.B.E., B.Sc., M.Inst.C.E. (Fellow): While Professor Feather and his colleagues had revolutionised the whole system of warfare as they had come to think of it in the past few years, an amazing feat to have accomplished in so short a time, everyone was thinking more of the future than of the past, and Professor Feather's lecture must appeal to them as engineers, and particularly as engineers interested in aeronautics. It was difficult for anyone ignorant of nuclear physics to foresee what life would be like in the immediate future; but one question which he felt qualified to ask was, what would be the outcome of all the work described; what would be its repercussions on their daily lives, and more particularly, on the design of aircraft, on the means of flight, on the altitudes at which they should be able to fly, and so on? Anyone familiar with the mechanics of flight must realise the great advantages which the aeronaut would derive from, say, the ability to be entirely independent of oxygen for his supply of propulsive energy. Flying at a height of 100,000 or 200,000 ft. would be a different affair from flying at low level; if Professor Feather could say that they should be able to take their stores of energy up with them and to be independent of the air through which they passed, he would clarify greatly many problems on which aeronautical engineers were now working.

MR. DAVIS: Could Professor Feather explain the system of electrons or bonds which every element possessed when coming into chemical combination with another? Each had a characteristic number. How was that explainable in terms of the internal structure of the atom?

PROFESSOR FEATHER: Briefly, but not completely, one might say that the chemical affinity in terms of bonds was connected with the number of electrons in the outermost shell. In general, the shell tended to be as complete as it could be, and there was a specific number of electrons which would form a complete shell at any stage in the building up of the element. To form a complete shell meant taking electrons from other atoms. The number of electrons by which there were too few, or the number by which there were too many, represented the number of chemical bonds. In oxygen the electrons were too few by two to form the complete shell. In water, one each from the hydrogen atoms went to make the shell in the oxygen atom complete.

MR. WEINBERG: Professor Feather had shown a curve indicating the activity of thorium, and there was another curve on the same slide which seemed to be complementary. Although it was said that the two added together to form a constant, it was obvious that they did not. Could Professor Feather explain that? Taking the two ordinates right at the beginning and towards the end, the sums were totally different.

PROFESSOR FEATHER: He had not noticed the difference in the sums. As the slides had been handed to him only just before the lecture, perhaps he had missed it. The complementary feature was representative of the state of affairs when two types of radioactive atoms were involved. If the curves showed some discrepancy it probably meant that other types of radioactive atoms were present as well.

MR. C. C. WALKER (Fellow): He referred to the use that could be made of the atomic process at very great heights, outside the atmosphere and said that at the lower altitudes the atomic bomb was surrounded by air; a great amount of heat was generated very rapidly and blast was propagated and knocked things down. If a bomb were burst at a height of, say, 100 miles, what would become of the energy released? There would be no air to become warmed up, there would be no medium on which the heat could perform. What form would the energy take,

having regard to the desirability of making use of it in some way?

PROFESSOR FEATHER: If an atom bomb were exploded outside the atmosphere, the energy would be in two forms; a small fraction as the kinetic energy of any fragments that were flying apart, and by far the greater part as the heat of the vapourised material which had formed the bomb. There would also be radioactive energy.

But the system which might be taken up to that height in order to propel an aeroplane would not be of the explosive kind. It would be a system in which the nuclear energy was appearing merely as heat, safely, in what might be called, for lack of a better term, a nuclear boiler. It would not be taken up unless it were developed to that stage of perfection. The action then would be purely mechanical. To perfect such a system would, however, require a long period of research, and fundamental difficulties might well prevent its ever being finally perfected.

A SPEAKER asked if Professor Feather would enlarge on the point that the energy was applied in a low temperature form in the aircraft.

PROFESSOR FEATHER: There would be a large amount of heat produced in a large system, but the heat was diffused throughout the whole system; there might be good reasons why it could not be allowed to get to a high temperature, reasons not depending on nuclear properties but on the large-scale properties of the substances used in constructing the system. It must be remembered that there would be the effects of intense radioactivity to be guarded against, as well as ordinary thermal effects.

MR. C. F. HODSON (Associate Fellow): Did Professor Feather consider that it would ever be possible to control the by-products without using a large amount of lead or other shielding substance around the apparatus?

PROFESSOR FEATHER: The two fragments into which the uranium nucleus split were always radioactive, so far as was known. Thus, not only did one produce energy in the form of the emission of particles, but one was left with so many radioactive atoms which, expressed in terms of radium, would represent perhaps tons, and certainly kilograms—and they did not like kilograms of radium. So long as the process as they knew it was used, he could see no hope whatsoever of being able to use it without providing shielding against the radioactivity of those atoms. A considerable amount of shielding would be necessary.

MR. WILSON: He referred to a curve showing the discrepancy between the sum of the masses of the protons and neutrons in the nucleus and the actual mass of the nucleus and asked if Professor Feather could cast any light on the extraordinary forces which held the neutrons together, having regard to the vast number of positive charges in very close proximity?

PROFESSOR FEATHER: The amount of energy required to overcome those forces could be calculated by multiplying the deficiency in mass by the square of the velocity of light.

MR. WILSON: He supposed, therefore, that the reduction of mass was due to the existence of the force which was holding the neutrons together?

PROFESSOR FEATHER: He did not know whether it was the cause or the effect; the two things were different expressions of one fact.

MR. C. G. WOODFORD (Associate Fellow): Was there any theory or speculation on the nature of the neutron? It was not matter, but it had mass, yet it had no charge, like the electron.

PROFESSOR FEATHER: The question bordered on the philosophic; there were

theories about the relation between neutrons and protons. He did not think they were very helpful to anyone who was not prepared to follow the mathematics; the attempt to express them in words did not give a clear picture. He felt that it merely led them a little further in the direction of mysticism. Briefly, however, the theory which was current, as a working hypothesis, was that there was some significance in speaking of neutrons and protons as essentially the same particles, but in different states of energy; and when the theory was fully elaborated, the difference in charge appeared also.

THE PRESIDENT: After commenting on the extreme interest of the lecture, he said the reference to the neutron might lead them to think of it as being perhaps applicable to some of themselves. It had no particular character; it had neither electro-positive nor negative charge. But apparently it performed a very useful function, because it bumped into things which had character and stimulated them, so that energy was given out. Therefore, he felt that those who were neutral, or neutron, in their characteristics could claim that they had a very useful part to play in society; and when some people in Industry or in Government Departments felt rather hurt because they bumped into them, they must remind them that they were of the neutron character!

On the President's proposal, a very sincere vote of thanks was accorded Professor Feather for his lecture.

THE ROYAL AERONAUTICAL SOCIETY

THE ELECTRICAL MEASUREMENT OF STRAIN

by

S. C. REDSHAW, M.Sc., Ph.D., M.Inst.C.E., F.R.Ae.S.

Dr. Redshaw took his degree at University of Wales in 1927. *He joined the Bristol Aeroplane Co., Ltd., as technical assistant, working on aerodynamics and structural strength calculations. In* 1931 *he joined General Aircraft as technical assistant and later became lecturer in Engineering at University of London. He is now Chief Engineer with Boulton Paul Aircraft, Ltd.*

A MEETING of the Royal Aeronautical Society was held on 28th March, 1946, in the Lecture Hall of the Institution of Civil Engineers, Great George Street, S.W.1, at which a paper on " The Electrical Measurement of Strain " by Dr. S. C. Redshaw, M.Sc., Ph.D., M.Inst.C.E., F.R.Ae.S., was presented and discussed. In the Chair, the President, Sir Frederick Handley Page.

THE PRESIDENT, introducing Dr. Redshaw, said that he had obtained his degree in civil engineering at the University of Wales in 1927. He had then become technical assistant to the Bristol Aeroplane Co., working on aerodynamics and structural strength calculations. In 1931 he had joined General Aircraft, Ltd., and had later become lecturer in engineering at London University. He was now Chief Engineer at Boulton Paul Aircraft, Ltd. He was a member of the Structures Committee of the Society and had been elected to Fellowship in 1944.

DR. REDSHAW then read his paper.

1.0. INTRODUCTION.

The electrical measurement of strain is an art which has been developed gradually over a considerable number of years. It will be realised that the electrical measurement of strain includes the electrical magnification and measurement of a strain indicated by a non-electrical pick-up, such as a mechanical strain gauge, as well as the case of pure electrical measurement in which the pick-up unit itself operates electrically.

The principal reasons for the popularity of electrical gauges are their ability to record data at a station remote from the point of application of the gauge, thus allowing gauges to be attached in positions which would be inaccessible for direct measurement by mechanical or optical means, and the high dynamic response of the electric gauge which renders the gauge invaluable for dynamic work.

The electrical form of measurement possesses great additional advantages because of the manner in which the measurement can be automatically recorded and methods are available for integrating, differentiating or averaging the strains. Means are also available for the simultaneous recording of strains from several pick-up units on a common chart or film having a suitable time base. An electrical gauge has been defined as a device which converts a physical magnitude into an electrical magnitude. All electrical circuits for the magnification and measurement of small movements involve some or all of the three basic electrical units of capacitance, induct-

ance and resistance and it stands to reason that any circuit component whose value can be changed by a mechanical phenomenon can be used for measurement purposes. Electric gauges may therefore be classified as being of the variable capacitance, inductance or resistance types or of the quasi-electric type such as the piezo-electric, acoustic and other specialised gauges of the mechanico-electric, electro-optic, thermo-electric or thermo-elastic types.

At the present time we are apt to think of the variable resistance gauge as being the only electrical type of gauge worth considering but it must be remembered that very fine work has been done with the quasi-electric types and in certain special cases it is probable that they will continue to be of service, although in my opinion there are few, if any, strain measurements which cannot be made satisfactorily by variable resistance methods.

2.0. DESCRIPTION OF GAUGES.

Before discussing measuring systems which incorporate pick-up units operating on electrical principles, it will be as well to review briefly two of the more important of the quasi-electric types which have been widely used.

2.1. *The Acoustic Strain Gauge*

The acoustic strain gauge has been used in a variety of forms in different countries, most types being developments of the Maihak gauge which has been widely used in Germany.

The principle upon which this type of gauge operates is that the frequency of vibration of a stretched wire clamped to the structure under observation is matched against the frequency of a second steel wire known as the reference gauge. A very sensitive form of this gauge has been developed by the Building Research Station[1] for measuring steady strains. The construction of the gauge is shown diagrammatically in Fig. 1. The gauge unit, which is attached to the point on the structure at which the strain is to be measured, consists essentially of two knife edges, one fixed and the other working on a slide, between which a wire is stretched. The wire is initially tensioned and is maintained vibrating at its natural frequency electrically. The wire passes between two pairs of laminated pole-pieces from two small bar magnets

1 R. S. JERRETT: The Acoustic Strain Gauge. Journal of Scientific Instruments. Vol. 22, No. 2, p. 29.

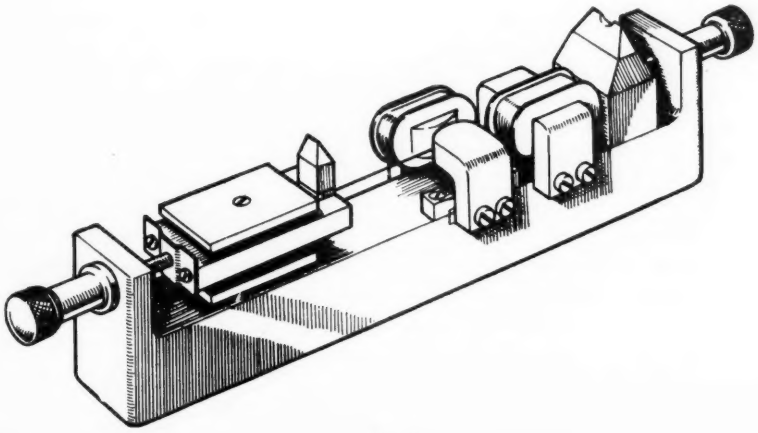


Fig. 1
Acoustic Strain Gauge

attached to the body of the gauge but insulated from it. Around the north-pole piece of each magnet is fixed a small coil of 1000 ohms resistance. The electrical circuit is such that one coil acts as an exciter coil and the other as a pick-up coil. When the set is switched on, small natural vibrations are set up in the wire which generate small oscillatory currents in the pick-up coil.

A second unit known as the reference gauge is also connected in the electrical circuit. This reference gauge is similar to the test gauge but it has no knife edges and is not attached to the structure; the wire is fastened to a tensioning screw which incorporates a calibrated dial. The arrangement for vibrating the reference gauge is the same as for the test gauge. The frequency of vibration of the wire can be changed by altering the tension applied to it by adjustment of the tensioning screw. In brief, the operation of the unit consists of matching the pitch of the note emitted by the test gauge and received in one ear-piece of a headphone worn by the operator, with the note emitted by the reference gauge, and transmitted to the other ear-piece. The two notes may, of course, be matched by the use of the tensioning screw on the reference gauge and the reading on the calibrated dial is a measure of the strain in the specimen. A recording set is capable of operating twelve gauges, each being operated in turn by the rotation of a switch.

Although the gauge is primarily intended for static work it has been successfully used for the measurement of strains produced by dynamic loads, but the technique involves the use of the double beam cathode ray oscillograph and the method of recording the results is somewhat lengthy. The unit is capable of recording a strain of 1×10^{-6} and can be used in remote places and operated from distances of several hundred feet. Any number of gauges can be controlled from the one amplifier and it is claimed that the gauge is easy to operate. It possesses a linear calibration but it is too large and heavy for use on aircraft structures.

2.2 *Piezo-electric Crystal Strain Gauge.*

It is well known that certain crystals exhibit electrical changes at particular regions when subjected to stress. The phenomenon has been exploited in the design of the piezo-electric crystal gauge. Rochelle salt and quartz crystals have accordingly been used for converting mechanical movements into electrical effects. Because of its much greater sensitivity, Rochelle salt can, in some cases, be connected directly to a cathode ray oscillograph without any pre-amplification. It is much less consistent in its behaviour than quartz, which makes it unsuitable for accurate work. The pick-up unit must be designed so that the crystal incorporated in it will be subjected to a pressure depending on the force to be measured. Owing to difficulties in mounting the crystal, only compressive loads can normally be detected, although tensions can be measured if the crystal is pre-stressed.

Crystals have been very successfully mounted for the direct measurement of pressure. When quartz crystals are used for strain gauge pick-ups pre-amplification of the piezo-electric effect is necessary and the chief difficulty in their use is the leakage of the generated charges. Although small crystals can be used, it is difficult to mount them into small, compact strain gauge units.

It is possible for several crystals which are located together so that they are all subjected to the same pressure to be wired in parallel so that the charges add up. The crystals are compressed between two discs held together by springs; the discs form one electrode and a ring attachment forms the other. The small voltage produced when the crystal is either further compressed or relieved is then amplified and displayed on a cathode ray oscillograph. Owing to the difficulties in preventing leakage of the generated charge and mounting the crystals, this form of pick-up is unsuitable for the measurement of strain. In general the use of a crystal gauge is unsatisfactory unless

it is used for indicating effects which are nearly instantaneous.

2.3. *Variable Capacitance Strain Gauge.*

The variable capacitance gauge operates on the principle that changes in the electrical capacitance between two metallic plates are caused either by a change in area or separation distance of the plates, or by the change in the di-electric constant of the material separating them. Gauges of widely different designs have been used successfully, but there is no standard design, and this type of gauge has proved of most use in specific applications.

When the unit forms a parallel plate condenser, the capacity is directly proportional to the di-electric constant of the di-electric and the area of overlap, and inversely proportional to the distance separating the condenser plates. Assuming no change is made in the di-electric, the capacity can therefore be varied by altering either the area of overlap or the separation of the plates. As the area is probably of the order of a square inch, while the separated distance is only a few thousandths of an inch, a given change in the capacity needs a far greater movement when the area is the variable than for the case when the separation distance is the variable. Therefore, it is usual for capacity changes to be caused by changes in gap, the changes in gap being restricted to small fractions of the total gap in order to prevent undue departure from linearity.

The capacity unit is attached to the structure at the point at which it is desired to measure the strain in such a manner that the fixed plate of the condenser would be fixed to the specimen at one point, while the diaphragm would be actuated by a rod attached elsewhere on the specimen. As a different arrangement, the two condenser plates can be quite independent of each other, their only connection being through the member under stress.[2] This latter form of attachment is shown diagrammatically in Fig. 2. The two parts of the gauge are rigidly fixed to the test specimen. It will be seen that surface strains

2 B. E. Noltingk: Survey of the Various Types of Electrical Strain Gauges. Inst. Automobile Engineers. Research Publication.

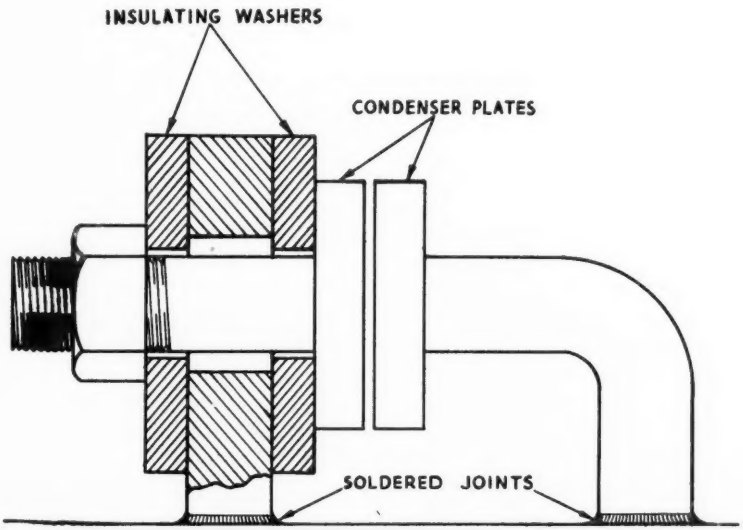


Fig. 2
Capacity Gauge (Gap Type)

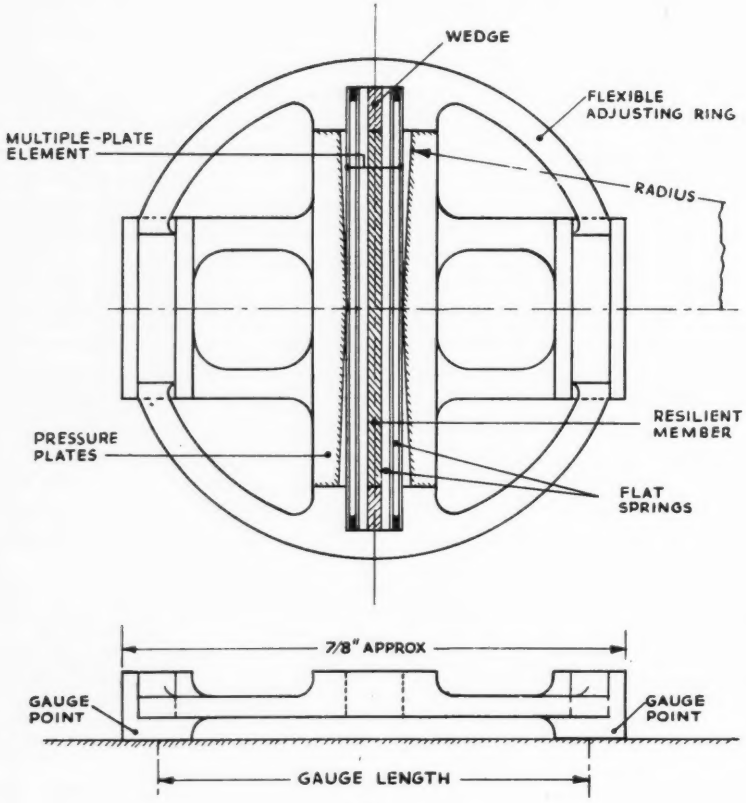


Fig. 3
Capacity Gauge (Area Type)

are immediately translated into capacity changes.

It will be readily appreciated that it is difficult to make a neat, efficient joint at the precise positions where the feet of the gauge are to be attached and, of course, in the majority of aircraft applications, where light alloy is almost universally used, a soldered joint would not be possible and an alternative form of attachment such as bolting or cementing would have to be devised. The condenser plates should be mounted as close to the specimen as possible in order to mitigate bending effects which would cause a lack of coplanarity on the condenser plates. As a first approximation only direct compression or tension will be measured, even if bending effects are present, because an increase in gap on one side is balanced by the decrease on the other side.

An exceedingly neat and ingenious capacity strain gauge, operating on the change in area principle, has been described by Carter, Shannon and Forshaw.[3] The gauge, which is slightly smaller in diameter than a halfpenny and only about twice as thick, is shown in Fig. 3. It has been described by its designers as being of the multiple plate tangent type and it consists of two pressure plates with faces curved to 16 inches radius, a flexible

3 B. C. Carter, J. F. Shannon, and J. R. Forshaw. Measurement of Displacement and Strain by Capacity Methods. Procs. Inst. Mech. Engs. 1945. Vol. 152, No. 2.

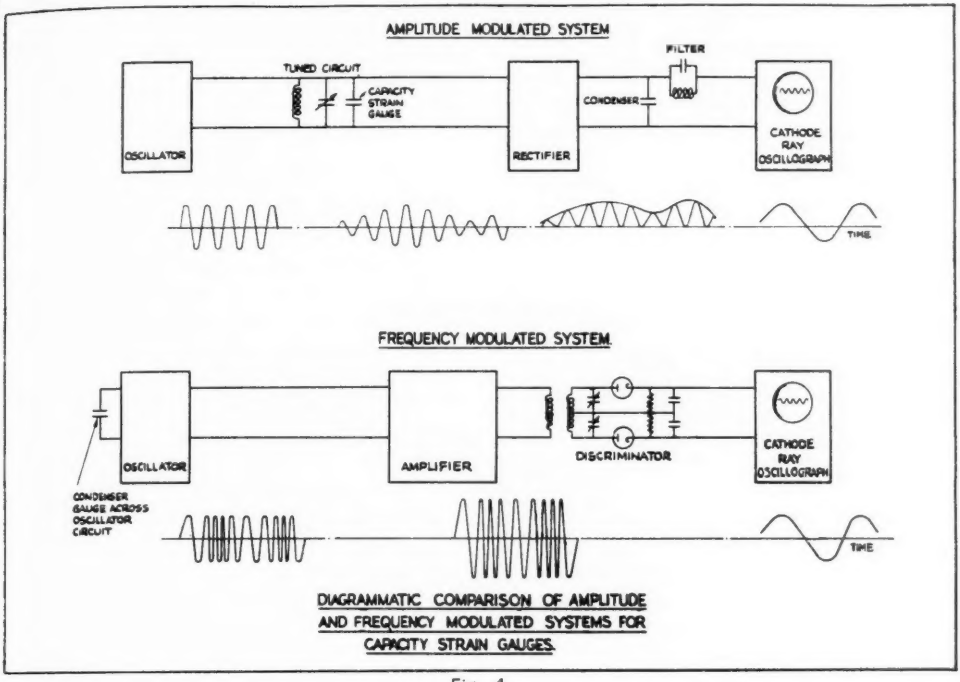


Fig. 4

adjusting ring and a multiple plate element. The plate element consists of two pairs of flat springs encastré at their ends. A thin layer of mica is inserted between the pressure plates and the outer springs and also between the springs. A resilient member is inserted between the two inner springs. The inner springs and the pressure plate are earthed and the two inner springs are connected in parallel, thus forming four condenser units.

The pressure plates transmit the strain between the gauge points to the multi-plate element which is thereby compressed and consequently curves round the circular arc. The capacity change is approximately 7 micro-microfarads per 0.001 inch deflection and the range is linear over 0.005 inches. The gauge is attached to the surface of the test specimen by a cement of the "Durofix" or De Khotinsky type. There are two principal methods for recording the signal from a capacity strain gauge, the amplitude modulated system and the frequency modulated system. The circuits and the modes of operation of these systems are shown diagrammatically in Fig. 4.

Strains caused by both static and dynamic loading can be successfully recorded by a capacity type strain gauge apparatus, but its chief use lies in specialised work and it has been most valuable in torsiograph applications. The strain gauge pick-ups were developed before the advent of the wire resistance strain gauge, which has now superseded them for most applications. Capacity gauges are valuable, however, in cases where the temperature rise of the specimen is very great and in cases where the range of strain to be covered would exceed the permissible strain for a wire resistance gauge. Great care has to be taken with the electrical circuits employed for picking up the signal from the gauge and particular care is required in damp atmospheres.

2.4. *Variable Inductance Strain Gauge*

Variable inductance strain gauges normally use a magnetic circuit in which the reluctance of the circuit is changed either by a change in the position of an iron armature or by a change in an air gap in the iron circuit.

An interesting gauge of the first type has been developed by Rotol Ltd., for the measurement of steady and alternating strains in aircraft structures. Briefly, the method consists of using an inductance variable with strain in one arm of an alternating current bridge circuit. The bridge is balanced to give nearly zero output at a value of inductance chosen as the "zero" and then at any other strain, and hence value of the inductance, the "out-of-balance" current of the bridge is a measure of the strain. An inductance variable with strain can be made to give an exceedingly high rate of change of inductance with strain and hence a high sensitivity. An alternating current bridge was selected because alternating current amplifiers with their high inherent stability can be used to obtain the necessary output for recording purposes with the required zero stability. A frequency of 11,000 cycles/sec. was used as this gives a reasonable size for the inductance and also a frequency some 5 or 10 times higher than the highest vibrating strain frequency that it is considered will be required. A voltmeter or a cathode ray tube can be used as the indicator of the bridge "out-of-balance" current and hence of the strain. When a voltmeter is used it is placed direct across the bridge output, without using an amplifier.

The performance of the gauge is such that with 1" effective length, strains of the order of 40×10^{-6} can be measured to within $\pm 5\%$, and strains of 100×10^{-6} to within $\pm 2\%$.

The gauge shown in section in Fig. 5 consists of a coil of enamel-covered copper wire wound on a "Tufnol" bobbin, which has a hole through the middle. This coil is mounted in a pot consisting of a tube section with a blanked-off end and a cap closing the other end. The blanked-off end has a central hole through which passes a plunger. The plunger and pot are mounted on feet which are attached to the test specimen. The leads from the coil are brought out through the pot to a small terminal block. The two units are cemented to the surface of the member under test, and since the pot and the plunger move

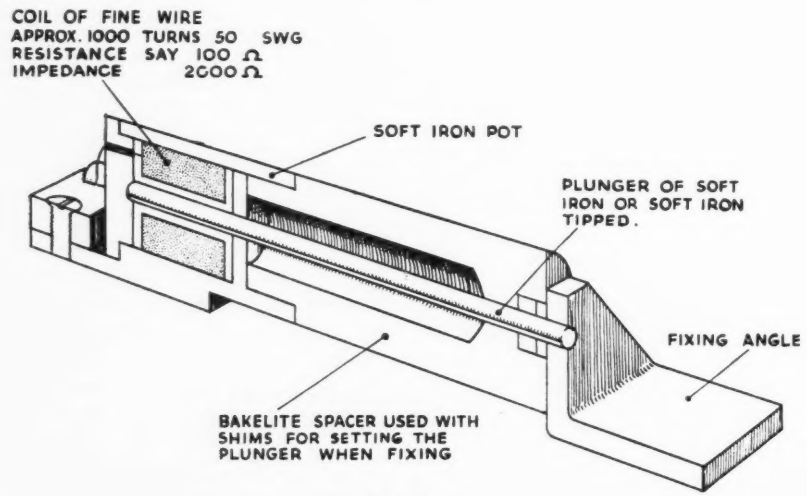

COIL OF FINE WIRE
APPROX. 1000 TURNS 50 SWG
RESISTANCE SAY 100 Ω
IMPEDANCE 2000 Ω
SOFT IRON POT
PLUNGER OF SOFT IRON OR SOFT IRON TIPPED.
FIXING ANGLE
BAKELITE SPACER USED WITH SHIMS FOR SETTING THE PLUNGER WHEN FIXING

Fig. 5
Inductance Gauge

with respect to one another in accordance with the variation of the gauge length, the air gap between the end of the plunger and the cap also varies in the same fashion. This gap is normally of the order of 0.0002″ and the gauge length measured from centre to centre of the foot is usually one inch.

It will be seen that the gauge can be regarded as an inductance whose magnitude depends on the gauge gap and hence is dependent on strain. The gauge is connected in a bridge circuit and the bridge is supplied with alternating current at a frequency of 1,000 cycles/sec. and at 60 volts. When steady stresses only have to be recorded, or where the rate of change of strain does not exceed an effective frequency of 1 or 2 cycles per second, the output of the bridge is fed direct to an A.C. voltmeter. In this case strain measurements involve only reading of the meter.

When steady and alternating strains together have to be evaluated, the bridge output is fed via a step-up transformer (15.1 ratio) to a normal cathode ray oscillograph with a single amplifier stage prior to the deflecting plates of the cathode ray tube. In this case, at the low sweep frequencies used, a band of a certain width is obtained on the screen of the tube; the width of this band is a measure of the strain, and is actually the peak-to-peak amplitude of the 11,000 cycles per second " out-of-balance " current of the bridge circuit. A moving film camera is then used to record this as a strain-time curve.

Inductance gauges would appear to be of particular use as vibration or accelerometer pick-ups and it is doubted if they will ever be used extensively for the measurement of surface strain.

2.5. *The Variable Resistance Strain Gauge.*

The variable resistance strain gauge, although probably the most recent newcomer to the field of electrical measurement of strain, has had more research and development, time and money spent on it than any other type. This class of gauge has now reached such a stage in its development that it has secured what amounts almost to a monopoly in the field of strain measurement. The first form of resistance strain gauge was made from flat carbon resistors about 1 inch long, 3/16 inch wide and about 1/20 inch thick. They were mounted on a resin-impregnated material to enable them to be cemented to the test specimen. Variations in strain of the member to which the gauge is attached will cause variations in the length of the carbon resistor which results in a variation in its resistance. The change in resistance is largely due to changes in contact pressure between the carbon particles of which it is composed. This type of resistor, which usually has a resistance of the order of 10,000-25,000 ohms, has been extensively used for measuring the strains in airscrew blades. It has certain disadvantages, inasmuch as it suffers from a reduction in strain sensitivity with a rise in temperature, it cannot be attached very closely to the surface of the specimen under examination, and it can only be applied to surfaces of comparatively small curvature.

It may be used for the measurement of both static strains and dynamic strains of a very high frequency of the order of 20,000 c.p.s. The gauge is more suitable for dynamic work because relatively large resistance changes are caused by changes in temperature and humidity. It is claimed that its dynamic sensitivity is practically unaltered by changes in temperature and humidity.

The earlier types of wire resistance gauges were made by forming a grid of fine diameter wire of the order of 0.001 inch diameter, moulded between layers of bakelised paper or moulded into a transparent thermoplastic resin of the methyl methacrylate type.[4] These early gauges were about 4 inches long by $\frac{3}{8}$ inch wide by 0.035 inch thick and appear clumsy in comparison with the wire resistance strain gauge used at the present time, which

4 A. V. DE FOREST and H. LEADERMAN: The Development of Electrical Strain Gauges. N.A.C.A. Tech. Note. No. 744.

is formed from a fine wire cemented to a thin paper. The thickness of these gauges is now about 0.005 inch, and they can be attached to a surface having a radius of curvature of as low as $\frac{1}{4}$ inch. As the resistance of wire is proportional to its length and inversely proportional to its cross-sectional area, any increase in length, with consequent decrease in cross section, will cause an increase in electrical resistance. If the wire is compressed, which is possible if it is adequately supported by the cement and thin paper to which it is attached, a decrease in resistance will occur. If a resistance gauge which is cemented to a structure is wired up in one arm of a Wheatstone bridge which is initially balanced, any change in resistance of the gauge will cause an out-of-balance current across the bridge circuit. This out-of-balance current can be measured by means of a galvanometer, or a cathode ray tube if oscillatory strains are to be measured. An elaboration of this basic form of measurement can be applied for all types of resistance strain gauge.

The versatility of the wire resistance strain gauge is such that it is no exaggeration to say that its applications are limitless. It possesses the enormous advantages of minute size and negligible inertia, and it must be noted that holes need not be drilled in the structure or clamps provided for its attachment; it can be operated from long distances and it will function on specimens having a high curvature. By suitably changing the recording apparatus it will measure either static, oscillatory or transient strains and it possesses a high rate of response with a linear calibration over the large strain range of zero to about $5{,}000 \times 10^{-6}$ and strains of 1×10^{-6} may be measured.

Like other keen weapons, however, the wire resistance strain gauge has two edges, and unless care is exercised in its use the results will be disappointing or misleading. The pick-up element, the gauge, is the simplest of all the electrical types and, in fact, is the most purely electrical in its operation of any gauge. To offset this, the recording equipment is, except in the simplest cases, rather elaborate.

It is clearly desirable that research establishments should keep their strain measuring methods as universal as possible, and the wide variety of their problems may call from time to time for the use of every weapon in their electrical armoury; but in my view an engineering firm engaged on structural research or development will find it more economical in time, staff and money to confine their attentions, where simple mechanical or optical methods cannot be used, solely to the wire resistance method.

In the remainder of this paper, some principles concerned with the use of wire resistance gauges will be discussed.

3.0. WIRE RESISTANCE STRAIN GAUGE TECHNIQUE.

3.1. *Factors influencing the Selection of the Wire for the Gauge.*

It can be shown that if a prism of uniform cross section and length L is strained to a new length $L+\Delta L$, its electrical resistance will be changed from an amount R, say, to $R+\Delta R$ such that, approximately:

$$\frac{\Delta R}{R} \Big/ \frac{\Delta L}{L} = 1 + 2\nu + \frac{\rho_\epsilon - \rho}{\rho_\epsilon} \quad . \quad (1)$$

Where ν = Poisson's ratio.

ρ = Specific resistivity, unstrained state.

ρ_ϵ = Specific resistivity, strained state.

A similar argument applies for the case in which the prism, which in the case of the wire resistance strain gauge is a wire filament, is subjected to a compression load, which it is able to sustain because of the support afforded to it by the cement and paper backing of low elastic modulus to which it is attached.

The ratio $\frac{\Delta R}{R} \Big/ \frac{\Delta L}{L}$ is known as the Gauge Sensitivity Factor and is defined as the

ratio of unit resistance change to unit strain change. It is important to keep this factor as high as practicable as it is clearly desirable, for measurement purposes, to have as large a unit resistance change for a unit strain change as possible.

De Forest has pointed out[5] that the gauge sensitivity factor is a little understood phenomenon. If Poisson's ratio applies for a small diameter wire the first two terms of equation (1) would give a gauge factor of approximately 1.6. It is not clear to what extent Poisson's ratio changes for fine wire or what is the value of the third term in the equation, but in actual practice there is a very large variation in the gauge factor and Baldwin-Southwark[6] give extreme values of -12.1 for nickel and $+5.1$ for 5 per cent. iridium platinum. The gauge sensitivity factor is not the only factor which has to be considered when selecting a wire for strain guage purposes. There are three properties which are important in the selection of the wire in addition to the gauge sensitivity factor: the resistivity, the temperature coefficient of resistance, and the ease with which the wire can be fabricated into gauges.

The resistivity of the wire is important, as it controls the size of the gauge. It is desirable to select a wire with as high a resistivity as possible so that the size of the gauge can be kept to a minimum.

For static tests which are of long duration, the temperature coefficient of resistance is very important, but it is unimportant for dynamic tests of short duration or when transient strains are to be measured. If a gauge wire is used which has a high temperature coefficient, the apparent strains caused by a temperature change may exceed the actual strains which are being measured unless the test is of short duration.

Last but not least, it is useless to select a wire which is difficult to form into a gauge, however desirable its other characteristics may be. Unfortunately, the four main requirements for a suitable wire are frequently in conflict and, as with so many other things in life, a compromise must be made.

Broadly speaking, there are two main classes of gauge wires, depending on whether good temperature characteristics are required or whether the test is of short duration and advantage can be taken of a gauge of high sensitivity factor. Unfortunately good temperature characteristics and high sensitivity factors do not appear together. Fig. 6 gives the principal properties of most of the wires in use at the present time. In American practice Advance wire is used for static work where temperature changes may be important, while Iso-elastic is used for dynamic tests of short duration. In this country wires of the Nichrome type appear to be almost universally popular, temperature effects being avoided as far as possible by the use of a compensating gauge in the bridge circuit. In almost all cases the wire is 0.001 inch in diameter although Nichrome gauges having a wire diameter as small as 0.0005 inch have been manufactured. Nichrome wire is extremely strong and fulfils the requirements for ease of fabrication.

3.2. *Methods of Gauge Manufacture.*

There are two main methods by which gauges can be manufactured, by drawing the wires into a zig-zag on a flat former or by winding the wire around a flat plate or a circular cylinder which is subsequently flattened.

The first method has been developed into an extremely high state of perfection in America by Baldwin-Southwark and several of the aircraft companies. Fig. 7 shows a very simple machine for hand drawing gauges. The wire from the spool passes between fine hooks which mesh closely at the beginning of the operation. On a platform which can be raised from underneath the needles a piece of thin rice paper is placed. Beginning at the side nearest the spool of wire, the needles are drawn back in turn

5 A. V. DE FOREST and H. LEADERMAN: Op. Cit.
6 ANON: The SR-4 Bonded Resistance Wire Strain Gauge: Baldwin-Southwark Bulletin No. 179.

PROPERTIES OF WIRES FOR RESISTANCE STRAIN GAUGES

TYPE	MATERIAL	APPROX COMPOSITION %	APPROX RESISTANCE OHMS/YARD FOR 0·001" DIA WIRE	APPROX TEMPERATURE COEFFICIENT OF RESISTANCE PER °C × 10⁶	APPROX GAUGE SENSITIVITY FACTOR $\frac{\Delta R}{R} / \frac{\Delta l}{l}$	EASE OF MANUFACTURE
COPPER-MANGANESE-NICKEL	MANGANIN MINALPHA	84 CU, 12 MN, 4 NI, 85 CU, 12 MN, 3 NI,	750 - 800 730	2 - 50 ± 3	0·5	FAIRLY EASY
CUPRO-NICKEL	ADVANCE CONSTANTAN COPEL EUREKA FERRY	55 CU, 45 NI, 60 CU, 40 NI, 55 CU, 45 NI, 60 CU, 40 NI, 56 CU, 44 NI,	850 - 900	± 20	2·0 - 2·1	FAIRLY EASY
NICKEL-CHROME	BRIGHTRAY NICHROME	80 NI, 20 CR, 80 NI, 20 CR,	1600 - 1800	100	2·1 - 2·3	EASY
NICKEL-IRON-CHROME	CHROMEL 'C' GLOWRAY	64 NI, 25 FE, 11 CR, 65 NI, 20 FE, 15 CR,	1900 1980	180	2·5	SLIGHTLY DIFFICULT
ELINVAR TYPE	ISO-ELASTIC	C, 36 NI, 8 CR, 4 MN, SI - MO V, BAL. FE	1450	300	3·2 - 3·5	DIFFICULT

Fig. 6

from one side, thus forming a zig-zag of wire. The platform is raised by means of a cam operated by the lever at the end of the apparatus so that the paper is in contact with the underside of the wire. The wires are then cemented to the paper by means of a suitable cement and the surplus wire cut off from the gauge. Tape or wire leads are then welded or soldered to the gauge wire ends and the exposed side of the gauge covered by another layer of paper, a suitable paper reinforcement being given to the junction of the leads and the wire.

A good gauge is manufactured in this country on the drawn method by H. Tinsley and Co., Ltd., who have developed a special machine for the purpose. A noticeable feature of the Tinsley gauge is the absence of the piece of paper on the second side of the gauge, although the ends of the gauge are reinforced. Baldwin-Southwark, on the other hand, cover the second side of their gauges with a thin piece of felt which affords it some protection against convection currents.

The winding method of manufacture has been mainly developed in this country by the National Physical Laboratory, British Thermostat Company, Rotol Ltd., and others. A simple winding machine which has given excellent results is shown in Fig. 8. The wire is wound on to a flat former covered with thin resin-impregnated paper by turning the handwheel which operates a fine pitch screw, thus causing a spiral winding of the wire. The wire is cemented to the paper and the ends parted off. A slight taper on the former

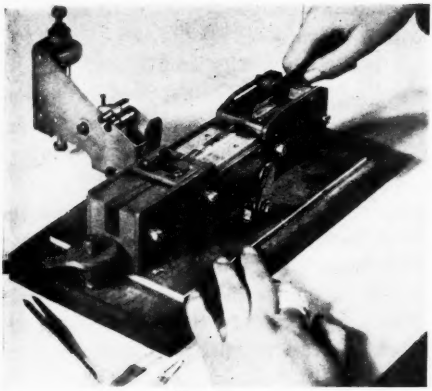


Fig. 7

allows the paper cylinder to be slipped off. The leads are welded to the gauge as before and the unit is protected with an external layer of resin-impregnated paper and the gauge is then pressed between heated platens to join the papers and wire together. There are several variations of this method, one of which is to wind the gauge on to a circular former which is subsequently pressed flat.

There are several excellent types of gauge manufactured commercially in this country, but frequently a gauge of special size or resistance is required, and the two simple units shown in Figs. 7 and 8 have been found to do all that is required in this connection. In my opinion it is infinitely preferable to

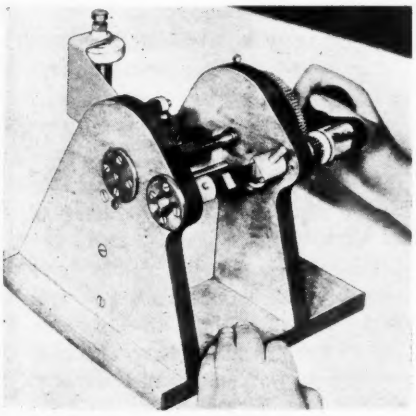


Fig. 8

weld the leads to the gauge and not to solder them together. If Nichrome wire is used for the gauge, Nichrome wire should be used for the leads, but a good soldered joint is difficult to obtain, even with the use of phosphoric acid as a flux. A simple welding apparatus which has been used successfully for welding the joint is shown in Fig. 9: with this apparatus Nichrome wire as fine as 0.00025 inch in diameter has been successfully welded to Nichrome leads.

Each method of gauge manufacture possesses different advantages. With the drawn gauge only one layer of paper separates the wire, which lies in one plane, from the speci-

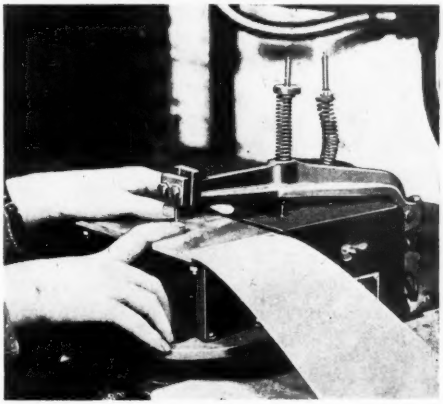


Fig. 9

men to which the gauge is attached. It is obvious that for satisfactory results the gauge wire should be as closely in contact with the specimen as possible to ensure that the gauge partakes of the full strain of the specimen without slip. A disadvantage of the drawn gauge lies in the fact that a small portion of the wire is at right angles to the main direction because of the end loops and, as the end loops cannot be cemented to the paper backing until the drawing hooks have been removed, these loops tend to be less taut than the remainder of the wire. Because of the length of transverse wire in the loops this type of gauge can pick up a transverse strain,

although this effect is probably negligible.[7] Due to this ineffective wire the gauge sensitivity factor will be lower than might be expected and unless the gauges are very accurately made this factor will not be consistent for a batch of gauges.

With the wound type of gauge the wire lies in two planes separated by two pieces of thin resin-impregnated paper. Unless the bonding is good when the gauge is made there is a danger that the gauge will not pick up the correct strain. This type of gauge is of necessity slightly thicker than the drawn type, but it possesses the advantage of avoiding the trouble caused by the loops and transverse wire which occurs with the drawn gauge. Naturally, unless the pitch of the winding is small, a transverse effect will be in evidence, as the wires will be inclined at a slight angle to the gauge axis. A very close winding is possible and by using enamelled wire very narrow gauges have been produced. The gauge sensitivity factor of this type of gauge is very consistent.

There is a considerable variation in the size and resistance values of both types of gauge, and provision can be made for sizes and resistances to meet individual needs. Broadly speaking, the usual resistance range is from 50-2,500 ohms with a variation in effective gauge length of $\frac{1}{4}$-1 inch.

Special rosette gauges, which incorporate three or four gauges oriented at a fixed angle to each other and mounted on the same base, are popular in America. From the direct strains determined by the three gauges of a rosette the complete state of two-dimensional strain at the point at which the gauge is attached can be determined. Good results can be obtained by the use of three independent gauges provided that a special jig is used for ensuring that the gauges are set in the correct position and, naturally, if an attempt is being made to deduce the strain at a point the gauges must be as small as possible.

It is now generally appreciated that the term "gauge length," as applied to the wire resistance gauge, does not correspond to the term as used for a mechanical extensometer.

If the gauge length of a mechanical strain gauge is increased, the total measured strain will be increased and hence the sensitivity to small strain changes. With the wire resistance strain gauge, however, an increase in gauge length without a change of resistance is useless, because no change has been made in the ratio $\frac{\Delta R}{R}$. All that would be achieved would be a summation of strain measurements over a greater length of the specimen, which is, of course, undesirable.

3.3. *The Attachment of the Gauge to the Specimen.*

The correct attachment of the wire resistance gauge to the specimen is of fundamental importance and considerable attention to this operation has been paid by numerous investigators. Poor adhesion of the gauge to the specimen probably accounts for a large percentage of the disappointments experienced in strain gauge technique. I have found that the following requirements for gauge attachment give satisfactory results:—

(1) Clean the surface of the specimen, at the point at which the gauge is to be attached, with acetone. A shiny surface may be abraded with 00 grade emery paper before cleaning.

(2) Coat the back of the gauge, or alternatively the specimen, with "Durofix" cement and place the gauge in the correct position. Excess cement may be squeezed out by rolling the finger from the centre outwards towards each end in turn. It is very important to remove air bubbles from between the gauge and specimen.

7 R. Baumberger and F. Hines: Practical Reduction Formulas for use on Bonded Wire Strain Gages in Two-Dimensional Stress Fields. Experimental Stress Analysis, Vol. 2, No. 1, p. 113.

(3) Allow the gauge to set for at least 24 hours before use. Accelerated drying by infra-red heating may be used but should be avoided wherever possible.

(4) If the gauge is to be used for any length of time, or in a position exposed to a humid atmosphere, it should be weather-proofed with a liberal coating of pure vaseline. In very bad humidity conditions, the gauges may have to be wax coated.

(5) The long copper leads connecting the strain gauge leads with the recording apparatus should be attached to the structure by adhesive tape in such a way that no load is placed on the gauge leads. The junction between the gauge leads and the copper leads should be covered with insulating sleeves and adhesive tape.

The importance of the correct installation of the gauges cannot be over-emphasised.

3.4. *Thermo-electric Effects in the Wiring System.*

If there is a temperature difference between the two soldered leads of a strain gauge the thermo-electric potential may lead to erroneous recordings.

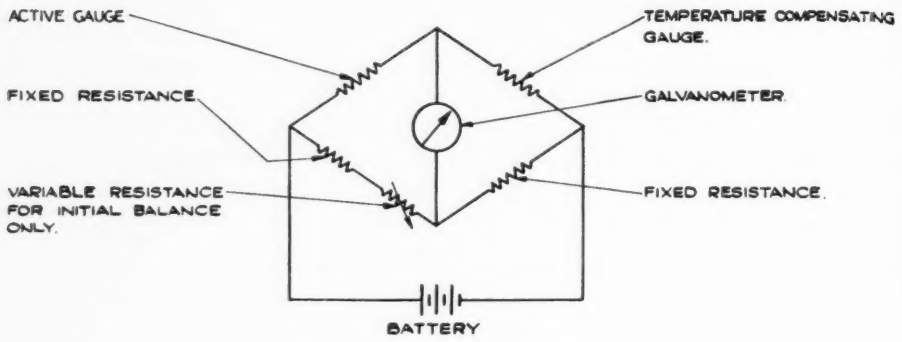
Ball gives details of the thermo-electric potentials of certain wire combinations.[8] An examination of his data shows that for certain combinations of materials care must be taken to ensure that the two gauge lead junctions are maintained at the same potential. If Nichrome is used for the gauge wire and leads and if copper is used for the main leads to the recording apparatus, thermo-electric effects will be negligible. If Advance wire is used for the gauge in conjunction with copper leads the position is unfavourable and a fictitious strain will be recorded if the joints are subjected to unequal temperatures. For this reason the lead junctions should be covered with insulating sleeves and tape.

3.5. *Strain Gauge Circuits.*

Basic strain gauge circuits for both static and dynamic work are exceedingly simple. For static tests either of the two simple adaptations of Wheatstone's bridge, see Fig. 10, is used. A strain gauge, which is attached to the specimen and known as the active gauge, forms one arm of the bridge. The other arm of the bridge is occupied by another gauge which is matched in resistance with the active gauge. (Gauges are supplied by the manufacturers in matched pairs to very close limits). This second gauge is known as the compensating gauge and it is attached to a piece of material identical with that to which the active gauge is attached; it will compensate for any pseudo-strain recorded from the active gauge due to a temperature change. It must be noted that it is not sufficient to use a gauge wire which has a low temperature coefficient of resistance; the gauge may suffer a change in strain if it does not possess the same linear coefficient of expansion as the specimen to which it is attached. Needless to say, the material to which the compensating gauge is attached must be unstrained and placed close to the active gauge. The other two arms of the bridge are fixed resistances, although it is usual to incorporate a variable resistance in one of these arms, for initial balancing, as shown.

A sensitive galvanometer and a battery are connected to the bridge in the normal manner. Any change in strain in the member to which the active gauge is attached will cause a corresponding change in resistance, throwing the bridge out of balance, the amount of out-of-balance being indicated by the reading on the galvanometer. The galvanometer reading is thus a measure of the strain in the specimen. The actual value of the strain can only be

[8] L. B. BALL: Strain Gage Technique. Experimental Stress Analysis, Vol. 3, No. 1, p. 1.



DIRECT READING METHOD

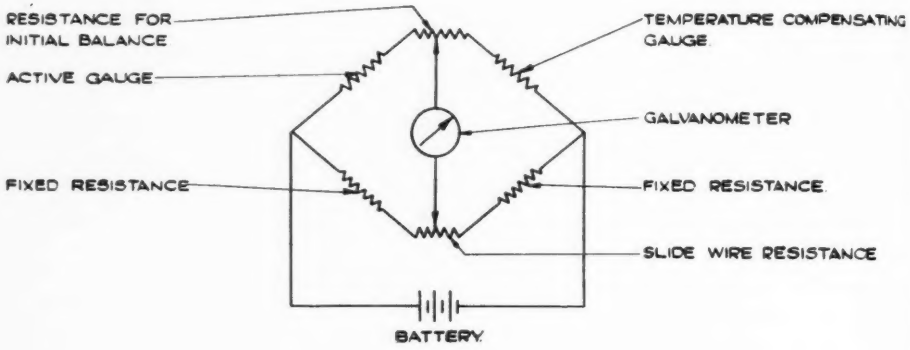


Fig. 10
Null Point Method

determined after the apparatus has been calibrated, a process which will be described in a later section. It will at once be seen that if the gauges suffer an apparent change in strain, due to temperature change, the result is cancelled out and no reading is shown on the galvanometer.

A disadvantage of this simple method is that the reading of the galvanometer is subject to fluctuations in the voltage of the battery and if the test is of long duration the readings may be in error if the battery voltage drops. This difficulty may be avoided by using a null point method by which a slide wire resistance is used to bring the galvanometer back to its zero position, the strain being indicated by the reading on a calibrated dial attached to the slide wire. This system renders the readings independent of battery voltage drop.

Before discussing the practical details of the set-up of the foregoing system it would be as well to discuss, briefly, the resistance requirements of the strain gauges themselves. Consider the simple system shown in Fig. 11. If, as a result of the straining of the specimen, the resistance of the active gauge is changed from R to $R+\Delta R$ the voltage ΔV across the recording instrument will be

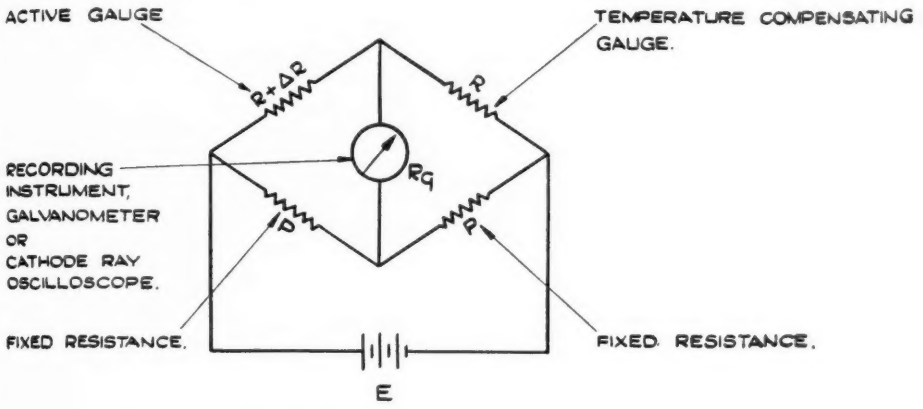
$$\Delta V = \frac{\mathrm{E}\ \Delta R\ R_G}{2R(2R_G+R+P)}$$

Where E is the bridge voltage, P the resist-

ance of the ratio arms and R_G the resistance of the recording instrument. If a cathode ray oscilloscope is used the value of R_G will approach infinity and we have

$$\Delta V = \frac{E\,\Delta R}{4R} \quad \text{approx.}$$

The cathode ray oscilloscope, which operates on voltage, therefore requires as high a value of ΔV as possible. To obtain this the bridge voltage should be as large as possible; this result is independent of the gauge resistance but it will be realised that, if a high bridge voltage is used, a high strain gauge resistance will be required in order to keep the current flowing through it to a low value. There are three factors which limit the high resistance value of the strain gauge; the ability to incorporate sufficient wire in a small space, the value of the insulation resistance which is in parallel with the gauge, and the fact that a high impedance input causes trouble from strong A.C. voltage



VOLTAGE ACROSS RECORDING INSTRUMENT

$$\Delta V = \frac{E\,\Delta R.\,Rq}{2R\,(2Rq + R + P)}$$

IF A CATHODE RAY OSCILLOSCOPE IS USED

$$Rq \rightarrow \infty$$

$$\Delta V = \frac{E\,\Delta R}{4R} \quad \text{APPROX.}$$

ΔV SHOULD BE AS LARGE AS POSSIBLE.

IF A GALVANOMETER IS USED.

$$Iq = \frac{E\,\Delta R}{2R(2Rq + R + P)}$$

FOR MAXIMUM SENSITIVITY.

$$R = 2\,Rq$$

AND $P \ll R$.

Fig. 11

pick-ups. For these reasons high resistance gauges are normally limited in this country and America to about 2,000—2,500 ohms. On the other hand, for static testing, when a galvanometer is used, it is desirable to have as large a current as possible to pass through the galvanometer. In this case, maximum sensitivity is achieved when the galvanometer resistance is half that of the strain gauge, and the values of the fixed ratio arms are made as small as practicable. A lower limit is set to the resistance of the gauge by the quantity of current which can pass through it without overheating the resistance wire.

It is difficult to set a limit to the current which can be carried, but for the normal English type of gauge made from 0.001 inch diameter Nichrome wire, a limit of 20 milliamperes is a reasonable guide. Thus, if a bridge circuit is composed of four arms of 50 ohms each, the maximum battery voltage should not exceed 2 volts, and as it is not practicable to use less than 1.5 volts this sets the lower resistance limit of the gauge at approximately 50 ohms. Higher resistance gauges can certainly be used without loss of sensitivity for static work provided that a corresponding increase in voltage is made, but with increased resistance an increase in gauge size is required to incorporate the increased amount of wire; on the other hand, the small size, low resistance gauge usually has a lower sensitivity factor because of manufacturing difficulties, and it is in view of these considerations that a compromise is made with a gauge having a resistance of 100-200 ohms.

Several strain gauges are often required on one structure, and in consequence a switching unit is needed so that each gauge can be connected in turn with the measuring instrument. It is necessary that the switching should take place in the galvanometer circuit, where the effect of contact resistance will have the least effect. The contact resistance should be small in comparison with the resistance of the gal-

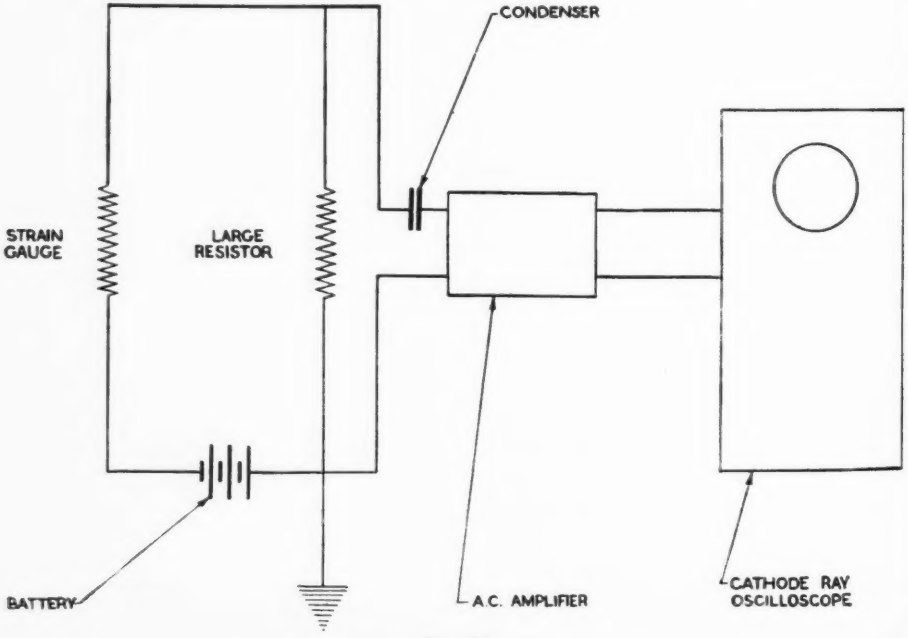


Fig. 12
Potentiometer Method for measuring Dynamic Strain

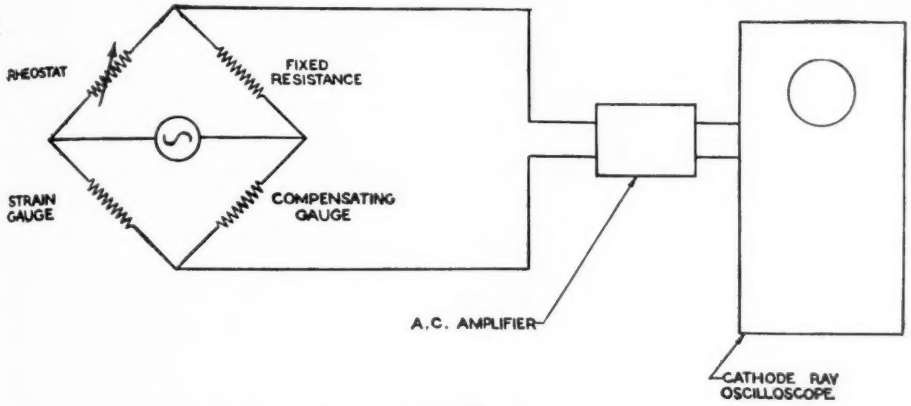


Fig. 13
A.C. Wheatstone Bridge Method for measuring Dynamic Strain

vanometer with which it is in series. Even so, switch contact resistance should be kept as low as possible and should in any case be kept constant. At the beginning of a test the constancy of the contact resistance should be checked by operating the switch several times and noting the galvanometer reading for each operation.

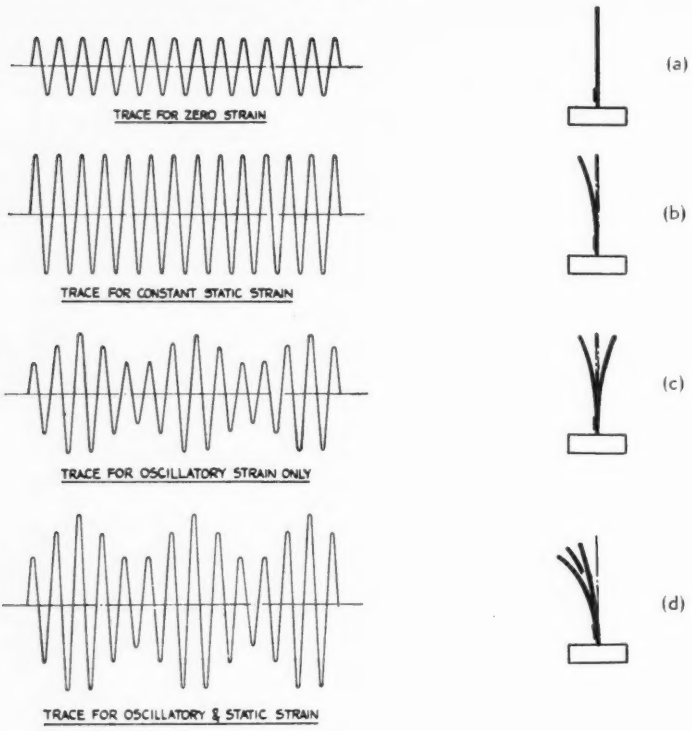
The signal received from the strain gauge may be amplified by means of a D.C. amplifier before it is transmitted to the galvanometer. The importance of using a reliable D.C. amplifier—one of the trickiest instruments in our electronic wardrobe—cannot be over-emphasised.

There are two principal methods for the measurement of dynamic strain. When records are required of alternating strains having a frequency in excess of about 10 c.p.s. a bridge circuit is not necessary, as temperature and voltage fluctuations will not affect the results during the short period of the test. The potentiometer method for measuring dynamic strain is shown in Fig. 12. It will be seen from the diagram that a change in strain gauge resistance under dynamic strain will be shown as a pulsating voltage across the ballast resistance. After amplification this voltage is applied to a cathode ray oscillograph and the amplitude of the trace measured. The condenser blocks out the steady component of the voltage appearing across the ballast resistance. It is necessary to have an earth connection to the system and this can be arranged conveniently in the position shown in the diagram. This method requires a high quality A.C. amplifier having a gain of about 100,000 times: the gain should be constant over the audio-frequency range of 10 to 10,000 c.p.s.

The foregoing method suffers from the disadvantage that oscillating strains only are measured, any steady strains being cut out. In order to measure both static and dynamic strains the A.C. Wheatstone bridge circuit shown in Fig. 13 can be used.

In this circuit a Wheatstone bridge circuit is energised by a simple valve oscillator. If the oscillograph sweep circuit is set for a low frequency, say 50 c.p.s., a wave is shown on the tube screen. By means of the rheostat in one arm of the bridge the amplitude of this wave can be reduced to its minimum value, Fig. 14 (a).

A static strain picked up by the active strain gauge will be indicated on the screen by an increase of the overall height of the carrier wave, Fig. 14 (b), and the change in height of the wave will be a measure of the strain in the specimen. An oscillatory strain will be indicated by a wave of the form shown in Fig. 14 (c) and a combination of static and



MEASUREMENT OF STATIC & DYNAMIC STRAIN.
FORM OF MODULATED WAVES.

Fig. 14

oscillatory strain will be displayed by a trace of the form shown in Fig. 14 (d).

A disadvantage of this system is that tensile and compressive strains are indistinguishable on the cathode ray tube; this does not, however, usually cause any serious inconvenience in practice.

3.6. *Calibration of Wire Resistance Strain Gauges.*

The calibration of a standard mechanical extensometer is a straightforward test, and from the result it is possible to state confidently that on a gauge length of so much, one division on the dial corresponds to a strain of so and so. Unfortunately, calibration of the wire resistance strain gauge is not so simple. In the first place, when the gauge is cemented to the test specimen, to remove it without damaging it is almost impossible. In fact it might be suggested that a gauge which can be removed without damage is not securely cemented! It is therefore impossible to attach a gauge to a calibrating specimen before setting it in position on the structure which is being tested. Secondly, the associated instruments require to be calibrated.

It has been mentioned previously that the strain gauges are normally supplied in resistance matched pairs, all the pairs in one box being made from the same spool of wire under the same conditions. In these circumstances it has been found that the gauge sensitivity factor, the ratio of unit resistance change to

unit strain, is sensibly constant for the batch and a reasonable procedure is to test one pair of gauges from each consignment. A static calibration test is usually all that is necessary and the simple apparatus of the type shown in Fig. 15 has been found to give very reliable results.

The calibrator consists of a light alloy beam of constant section which is subjected to four-point loading so that a constant bending moment is imposed on the beam between the two central points. It is advisable for some type of roller to be incorporated at one of the supporting points to avoid a direct tension being imposed on the beam when it is loaded. A load is applied by means of a central straining device.

One gauge of the pair to be calibrated is cemented to the upper surface of the beam while the other is cemented to a piece of material, identical to that from which the beam is composed, and laid closely alongside the active gauge. The two gauges are then wired into the Wheatstone bridge circuit in the normal manner. The beam is then deflected by means of the straining device and the deflection of the central point of the beam relative to the support joints is measured by means of an accurate dial gauge; at the same time the dial readings or galvanometer deflections, whichever form of instrument is used, are recorded.

It can be shown that, providing the distance between the supports is large in comparison with the central deflection, the strain in the beam ϵ can be expressed as

$$\epsilon = \frac{4dh}{s^2} \quad . \quad . \quad . \quad (4)$$

where d = depth of beam.
s = distance between supports.
h = central deflection.

As the depth of the beam and the distance between supports can be determined at the time when the beam is made, the measurement of the strain simply consists of recording the central deflection for corresponding strain gauge apparatus readings. The process is repeated for successive deflection increments. It will be noted that it is not

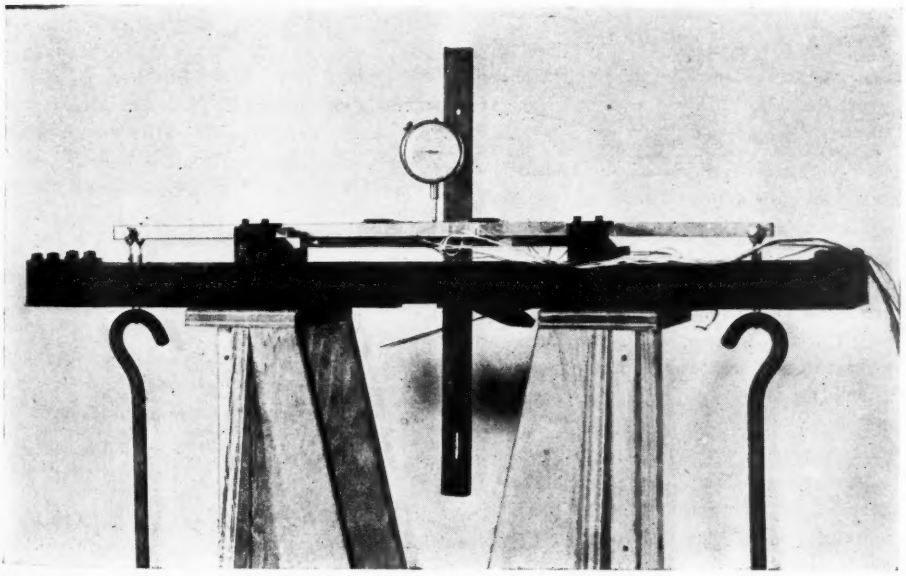


Fig. 15

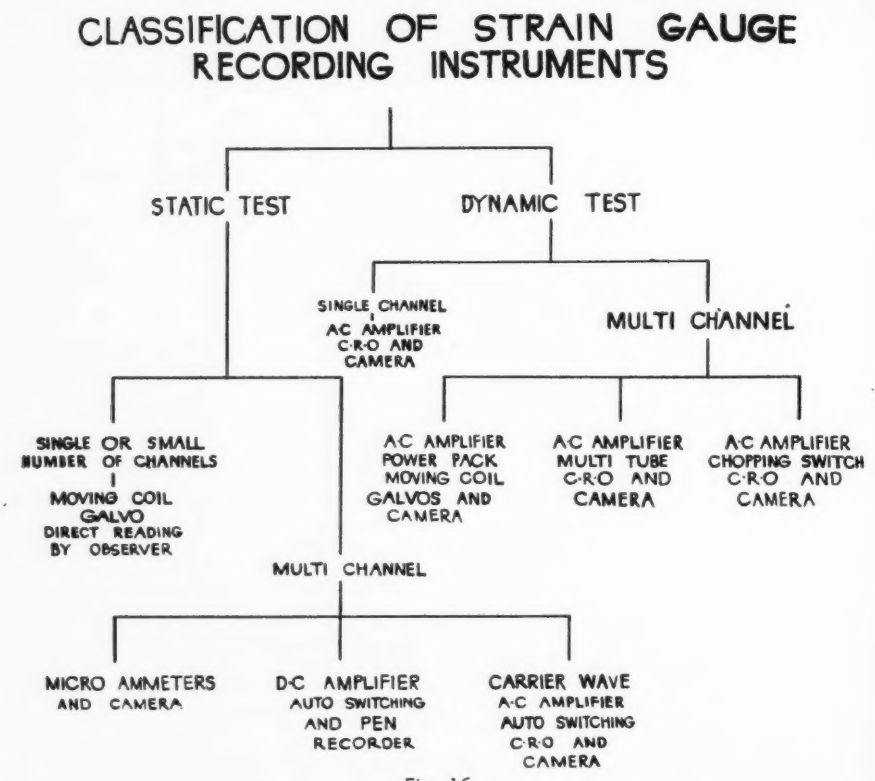


Fig. 16

necessary to measure the load applied to the beam.

In this manner the strain corresponding to one division on the dial on the strain gauge apparatus can be determined, and this figure may be accepted for all the other gauges in the batch. The beam is then reversed on its supports, so that the upper surface now becomes the lower surface, and the test is repeated. In this manner the strain gauge is calibrated both in tension and compression. It will be observed that by the foregoing method the strain gauge sensitivity factor is not determined nor, in fact, need it be for the interpretation of the results. Should it be desired to determine this factor accurately the following method may be applied.

With the gauge positioned as for calibration in compression, an accurate high value resistance, about two thousand times the resistance of the strain gauge, is placed in parallel with the active gauge in the Wheatstone bridge circuit. The bridge circuit is balanced without the high resistance switched into the circuit and then, with the beam still unstrained, the high resistance is brought into the circuit and the reading on the strain gauge recording apparatus is noted. From the previous experiment, dial readings against a known strain have been recorded and the new reading corresponds to some apparent strain which can be read from the calibration results.

$$\text{Now } \frac{\Delta r}{r} = \frac{r}{R+r} \quad . \quad . \quad . \quad . \quad (5)$$

Where r = resistance of strain gauge,
Δr = apparent increase in resistance of strain gauge.
R = resistance of large resistor.

Then if e is the value of the strain which gives a dial reading corresponding to that given by the ratio $\frac{r}{R+r}$ the strain gauge sensitivity factor will be

$$\frac{\Delta r}{r} \Big/ \frac{\Delta L}{L} = \frac{r}{e\,(R+r)} \quad . \quad . \quad (6)$$

Needless to say, a high quality decade resistance box should be used for this experiment.

3.7. *Recording Instruments.*

A review of the previous section shows that there are three main circuits for the measurement of strain by the wire resistance strain gauge technique; the D.C. energised Wheatstone bridge for static strain measurements and the potentiometer and carrier wave methods for dynamic strains. It must be appreciated, however, that there is a great variety in the actual detail of the circuits, and doubtless other circuits have been and will be designed for particular purposes.

Providing care is taken in mounting the gauges and in wiring up the circuits—really good soldered joints must be insisted upon in all places—no difficulty should be experienced with the technique. Unfortunately, the recording instruments are often very elaborate and expensive and require experienced personnel to handle them. In Fig. 16 an attempt has been made to classify the recording instruments according to the nature of the test on which they are employed.

For static tests, if readings are required from either a single gauge or a small number of

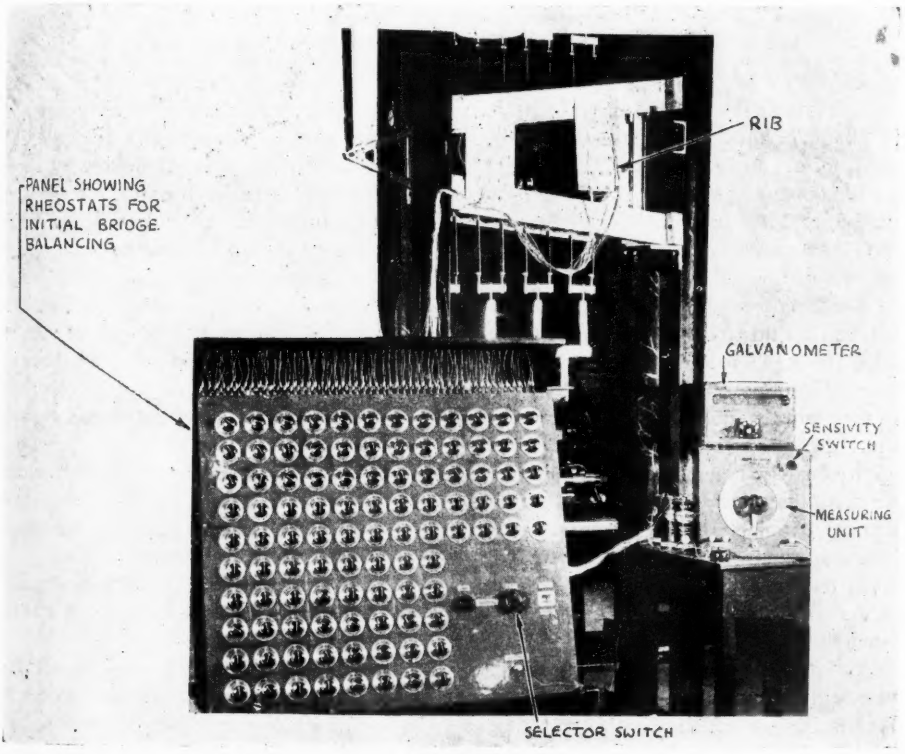


Fig. 17
100 Channel Static Strain Measuring Unit arranged for Rib Test

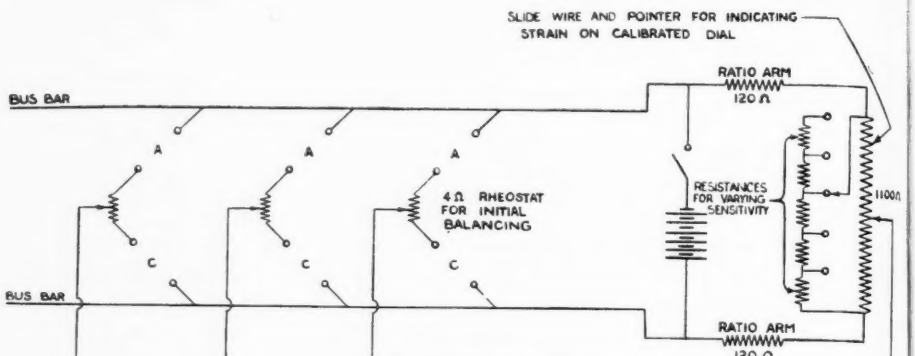


Fig. 18

100 Point Set for measurement of Static Strains (Null Point Method)

gauges, a moving coil galvanometer of the normal laboratory type having a sensitivity of about 75 mm. per microampere with a period of about 2 secs. is admirable. Either the direct reading or null method may be employed, and if a small number of gauges are used the observer can scan each gauge in turn by means of his selector switch. A limit is set to the number of gauges which can be scanned by this method because of the time taken for each recording. In a number of cases it is not possible, or at least inadvisable, to maintain the applied load for more than a short period and as each reading takes approximately a quarter of a minute, it will be seen that it is often impossible to use manual scanning satisfactorily.

A one hundred channel set developed for static tests is shown in Fig. 17. This figure shows the apparatus being used for the strength test on a rib. In this instance only thirty-four gauges were attached to the specimen and the null point method of measuring the strains was used. In this test the observer was able to scan the thirty-four channels manually in about six minutes. Fig. 18 shows diagrammatically the general arrangement and circuit of the apparatus.

Where several channels have to be recorded simultaneously, or at least in a very short space of time, there are three main methods in use at the present time. In the first case each gauge bridge circuit is connected to a separate microammeter, the microammeters being mounted together on a panel in a vibration-proof case. A camera is set up in such a manner that it will photograph the battery of microammeters as required by the observer. This method, which was developed by the National Physical Laboratory, imposes no limitation to the number of channels which can be recorded, as several panels and cameras can be used. It possesses the disadvantage that the microammeters are relatively insensitive and it is therefore unsuitable unless high strains are to be recorded or a D.C. amplifier is used.

This method has also been used for the measurement of steady strains in aircraft in flight, the microammeters being mounted in an automatic observer. This is not a desirable application of the method except in cer-

tain circumstances, because the microammeters have a slow period and are only suitable for steady strains; besides which they are delicate instruments which dislike vibration and acceleration.

An example of this method is provided by a test made on a Defiant aircraft for the purpose of determining the pressures in the hydraulic undercarriage retracting jacks during retraction, so that an estimate of the horse-power needed to retract an undercarriage of this type during flight could be made. Fig. 19 shows the undercarriage of the aircraft with four pressure units installed (up and down hydraulic lines of port and starboard legs). The movement of the legs was measured by a spirally-wound rheostat attached to the jack. Closure of the jack caused a contact to slide along the rheostat, thus causing a change in resistance. The rheostat was wired into a Wheatstone bridge circuit; the pressure unit pick-up, which incorporated wire resistance strain gauges, is described later in this paper.

The signals from the pressure units were recorded by microammeters mounted in an automatic observer carried in the aircraft. Airspeed, altitude and time were simultaneously recorded by the camera of the automatic observer, which took approximately five frames per second. One of the frames taken by the automatic observer is shown in Fig. 20. The developed film was passed through a projector and from the dial readings the curves shown in Fig. 21 were plotted. The information given in these figures, together with the geometry of the undercarriage, enabled the horse-power for ground and air retraction to be assessed.

The method of using D.C. amplification

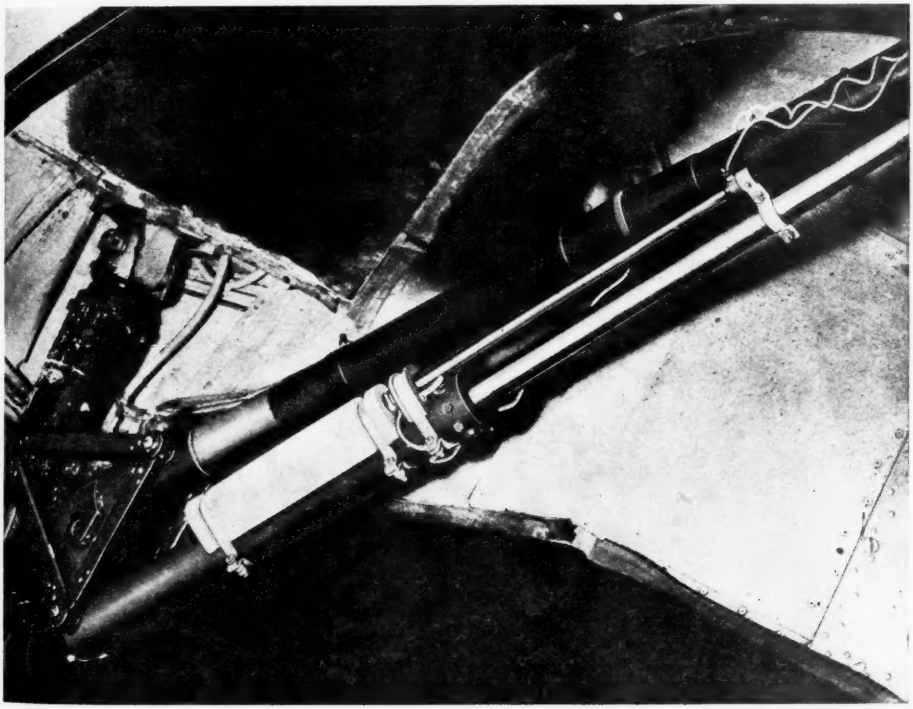


Fig. 19 (Part I)

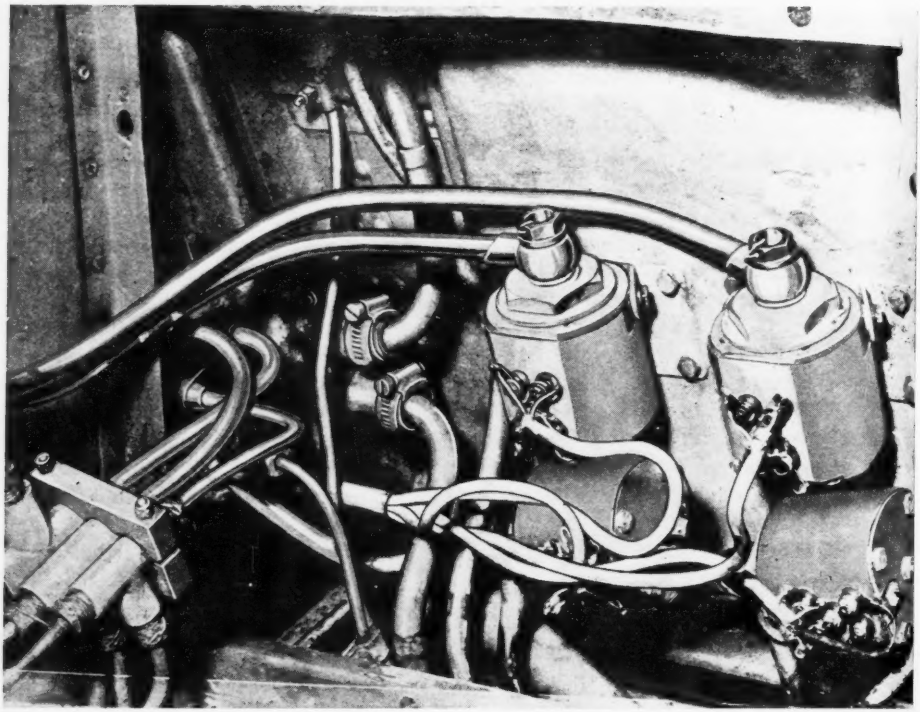


Fig. 19 (Part II)

with automatic switching and pen recording has been developed most successfully by Mr Eric Jones, of the Royal Aircraft Establishment, and Fig. 22, for which I am indebted to the Director of the Royal Aircraft Establishment, shows the complete static strain measuring equipment. The unit is designed for 100 channels, the initial balancing rheostats for which can be seen at the top of the panel. A high speed Post Office automatic telephone switch scans the board, after each gauge is initially balanced, at a signal from the operator. The out-of-balance current from each gauge bridge circuit is amplified by means of a D.C. amplifier, the signal being recorded by an automatic pen recorder on a linear moving chart. The pen recorder is so designed that the successive signals received from each gauge are recorded alongside each other each time the load is increased on the specimen. This is a great advantage, as any deviation from the linear portion of the stress-strain curve can be immediately detected. The bridge circuits are energised by transformed current from the mains and a voltage regulator ensures that the bridge current is kept constant. Excellent results have been obtained by the use of this instrument, despite the fact that a D.C. amplifier has been used.

In America, similar systems have been used successfully, the recording chart often being of the circular rotary type; and in many cases the null point method is used by the incorporation of automatic rebalancing.[9]

The third method of multi-channel recording for static strains is a system borrowed from dynamic strain measuring techniques. In this method the carrier wave system is

[9] W. F. Gunning and E. G. Van Leeuwen: Resistance Wire Strain Gage Equipment for Static and Dynamic Testing. Product Engineering, July and Sept., 1945.

used, each bridge circuit being energised by means of a simple valve oscillator of the type previously described. An A.C. amplifier is required and the signal from each channel is displayed on the cathode ray tube in turn by means of a high speed automatic telephone switch. A continuously moving film camera records the traces. This method is only in the experimental stage and possesses several drawbacks; it is not possible simultaneously to view the screen and photograph the results, and hence a whole test may be wasted if the camera or any other portion of the apparatus fails to function satisfactorily. A further disadvantage lies in the fact that if a strain gauge becomes inoperative during the test the sequence of results will be upset, and the discontinuity on the cathode ray tube may upset the results from neighbouring gauges. An advantage of the method lies in the fact that inexpensive apparatus and a standard cathode ray oscillograph can be used.

For dynamic tests the carrier wave system appears to be the best to use and, as for static tests, the instruments may be divided into two groups according to whether single channel or multi-channel recording is required. For single channel work the normal A.C. valve oscillator energised bridge circuit, with an A.C. amplifier, transmitting to the standard cathode ray oscillograph can be used. The Mullard G.M. 3156 cathode ray oscillograph has given good results in this connection, the display from the tube being photographed by a moving film type of camera. Typical equipment of this type is shown on test in Fig 23.

To meet the requirements for a suitable instrument for the recording of oscillatory and transient strains the Electronic Recorder, shown in Fig. 24, was designed and built to

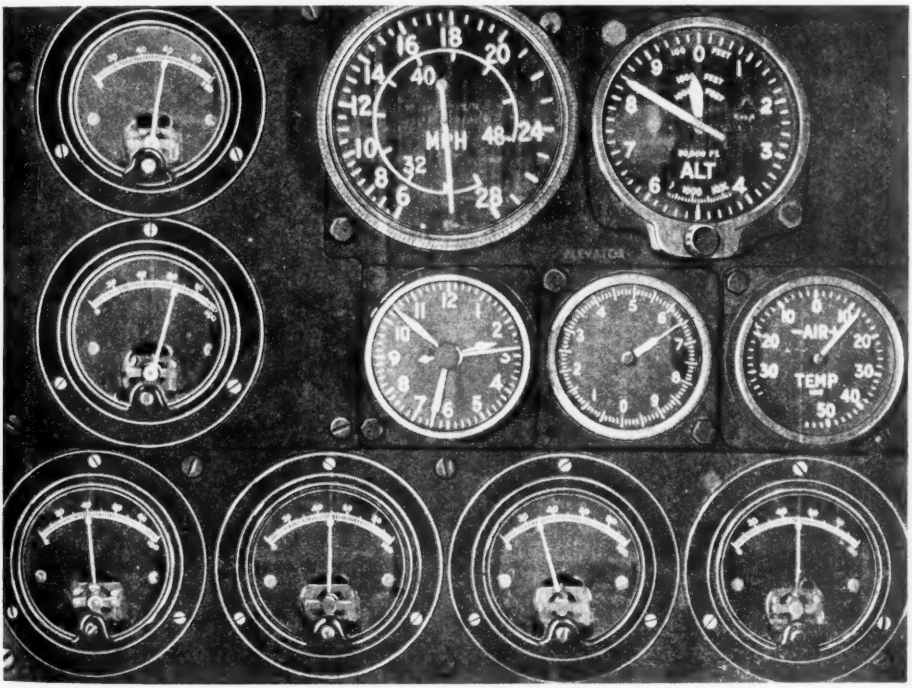


Fig. 20

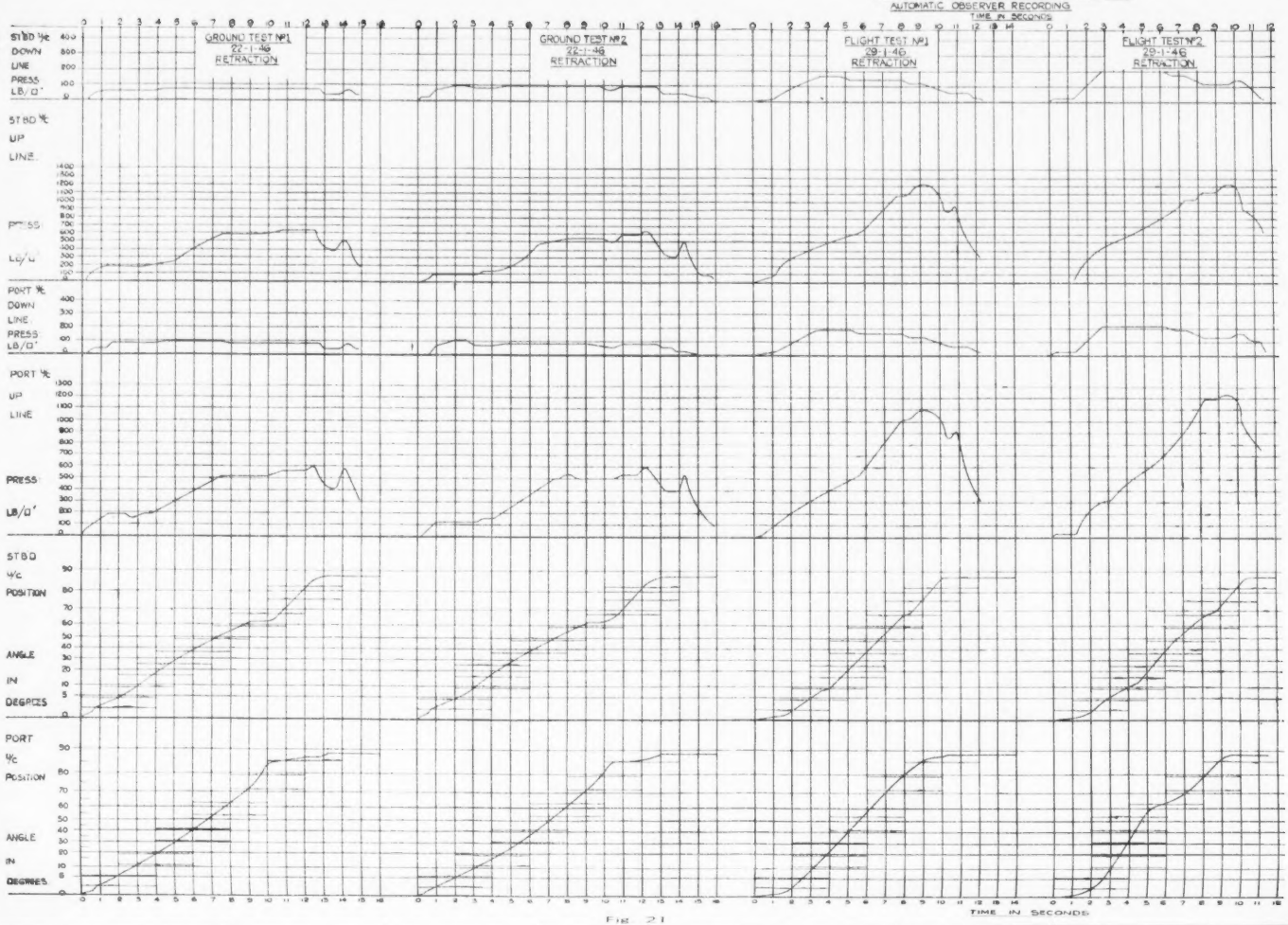


Fig 21

S. C. REDSHAW

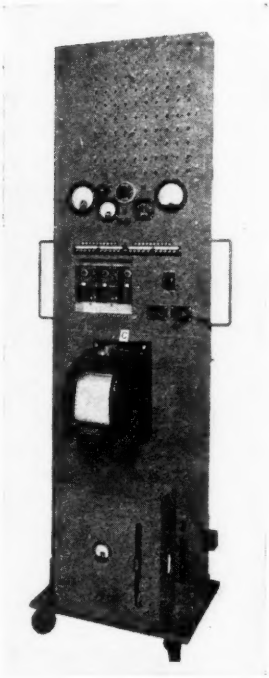


Fig. 22

Boulton Paul Aircraft Ltd. requirements by the Radio Gramophone Development Co., Ltd. This instrument, which operates on power mains voltage, consists essentially of an oscillator capable of producing audio-frequency sinusoidal wave-forms within a range of approximately 1,000 — 5,000 c.p.s. The oscillator output energises a balanced Wheatstone bridge network, one arm of which consists of an "active" wire resistance strain gauge which is incorporated in the pick-up, which is attached to the specimen under examination. Any change in strain in the strain gauge causes a change in its electrical resistance and corresponding proportional "unbalance" voltages appear across the bridge network. These are amplified by two separate amplifiers connected in cascade, the output from which is subsequently applied to the "Y" plates of the cathode ray oscillograph. As an aid to visual inspection a linear time base is made to operate on the "X" plate of the cathode ray oscillograph and the display then resembles a modulated carrier wave with the bridge supply voltage acting as a carrier. To facilitate timing of the display, the "X" axis can be calibrated, when required, at intervals of 0.02 sec. For photographic work the time base is switched off and a moving film camera, with its own time marking device, is attached to the instrument.

The eight units of which the recorder is composed, shown in block diagram form in Fig. 25, are conveniently mounted in a steel angle framework provided with rubber tyred wheels for ease of transport, while to facilitate repair and maintenance each unit is designed as a sliding tray located in the framework. Electronic screening is provided by enclosing the frame with quickly detachable ventilated steel panels secured by "Oddie" fasteners to the frame members, the front portion having hinged doors for access to the controls.

The oscillator feeds the Wheatstone bridge with a choice of five frequencies, their values being approximately 1,000, 2,000, 3,000, 4,000, and 5,000 c.p.s.

The cathode ray tube, which is of the television type, has an effective diameter of 12 inches and gives a particularly fine spot definition.

The pre-amplifier is fitted with a gain control to regulate its output. Its function is to amplify the minute voltages fed to it from the bridge; because of its high sensitivity it has to be completely screened to avoid extraneous "pick-up."

The function of the calibrator is to produce a timing mark on the display of the cathode ray oscillograph every fiftieth of a second. It consists of a square wave generator and a phase inverter and amplifier. A negative-going square wave of frequency 50 c.p.s. is fed from this unit to the grid of the cathode ray oscillograph, causing a brief blackout of the beam every 0.02 sec., thus providing a

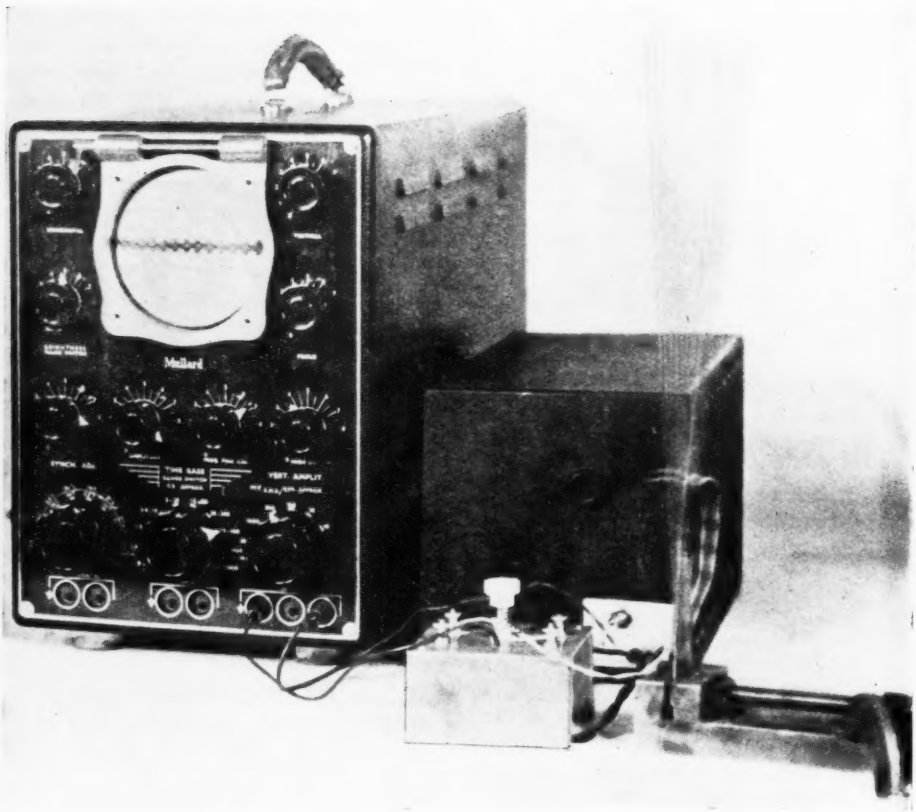


Fig. 23

timing calibration on the display. The accuracy of the timing is of the same order as that on an electric clock driven from a frequency-controlled mains supply. The H.T. for the tube is obtained from a voltage doubling circuit giving approximately 6,000 volts. The deflection sensitivity of the cathode ray oscillograph may be expected to be very nearly one inch per 100 volts. The power supplies unit contains two conventional power packs for feeding the oscillator, preamplifier and calibrator; the whole unit is designed to work from 230 volt, 50 cycle A.C. mains.

An "Avimo" camera, for recording the tube display on a 70 mm. film, has been modified for use with the large size tube employed, by fitting a special Cooke anastigmat $3\frac{1}{2}$" $\times$ f 1.5 lens. The excellent definition of the tube spot enables a good photographic record to be obtained on the extremely high speed, minimum contrast panchromatic film employed. The camera spools allow a maximum length of 80 ft. of film to be used and a choice of five different film speeds is available.

The camera incorporates a mercury vapour discharge lamp for time marking; provision is also made for direct viewing of the screen with the camera in position.

The use of the large size cathode ray tube incorporated in the Electronic Recorder, besides giving a large trace and fine spot definition, possesses the additional advantage that by means of a suitable high speed switching

device a number of traces can be displayed simultaneously on the tube.

Among many applications, the Electronic Recorder has been successfully used to measure the fluctuating pressures in the hydraulic pipe lines of a gun turret while undergoing ground running tests; the motion of the slide of the turret hydraulic generator was also measured. The test arrangement is shown in Fig. 26. Both the pipe pressures and the slide movement were measured by means of strain gauges mounted in the pick-up units shown in Fig. 27.

The pressure unit, which is of the type used in the flight test previously described, consists essentially of a chamber machined from a solid block of steel; this chamber is coupled into the oil pipe line and the oil pressure causes the thick circular plate, which comprises one wall of the chamber, to deflect. Between the plate and a stirrup attached to it, a small circular ring is imprisoned; thus a

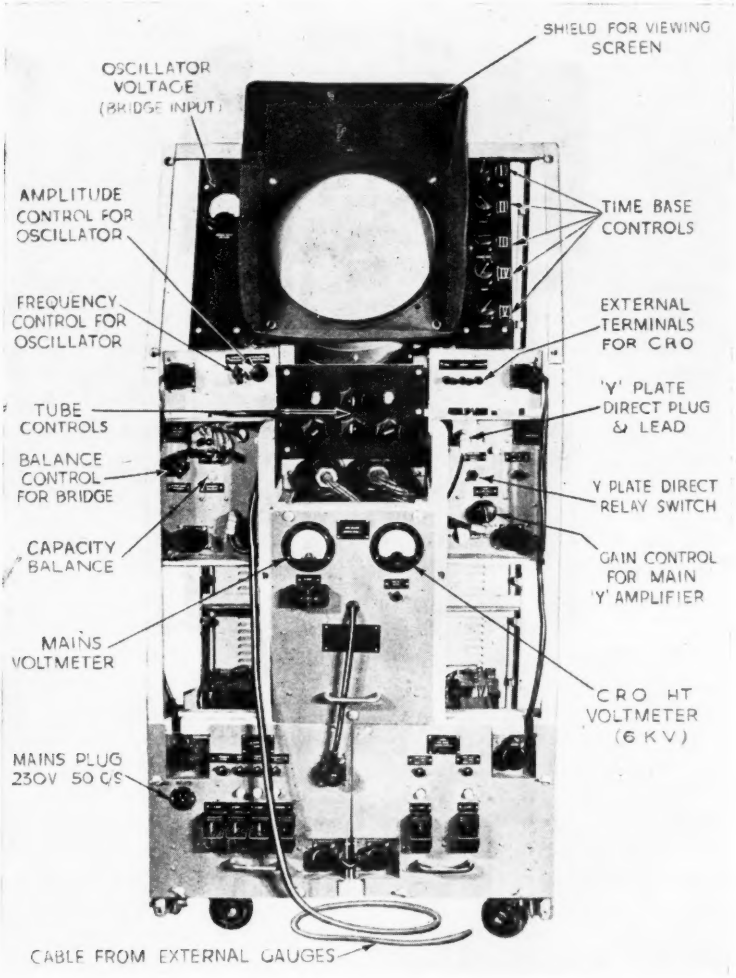

SHIELD FOR VIEWING SCREEN
OSCILLATOR VOLTAGE (BRIDGE INPUT)
AMPLITUDE CONTROL FOR OSCILLATOR
FREQUENCY CONTROL FOR OSCILLATOR
TUBE CONTROLS
BALANCE CONTROL FOR BRIDGE
CAPACITY BALANCE
MAINS VOLTMETER
MAINS PLUG 230V 50 C/S
CABLE FROM EXTERNAL GAUGES
TIME BASE CONTROLS
EXTERNAL TERMINALS FOR CRO
'Y' PLATE DIRECT PLUG & LEAD
Y PLATE DIRECT RELAY SWITCH
GAIN CONTROL FOR MAIN 'Y' AMPLIFIER
CRO HT VOLTMETER (6 K.V.)

Fig. 24

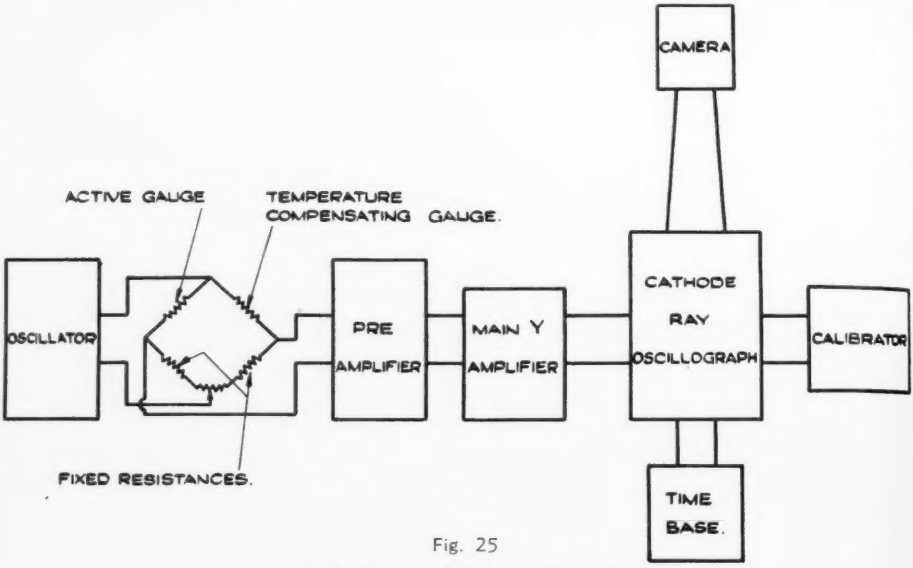


Fig. 25

Block diagram of Electronic Recorder

deflection of the plate causes a deformation of the ring. Four strain gauges are attached to the ring, two on the inside and two on the outside. These gauges are wired to form a complete Wheatstone bridge circuit. The advantage of the ring method is that the circuit is automatically temperature compensating and as all four gauges are active the signal will be four times as large as that recorded from one gauge. The deflection of the plate is approximately 0.003 inch for a pressure of 3,000 lb./in.^{2}. Pressure changes of the order of 20 lb./in.^{2} can be recorded.

The slide movement measuring device consists of a tapered cantilever plate, the free end of which is attached to the generator slide. A strain gauge is cemented to either side of the cantilever, the gauge being wired in a Wheatstone bridge circuit. Once again automatic temperature compensation is secured and the signal from the two gauges is additive. Sample recordings taken by means of These instruments are shown in Fig. 28.

For multi-channel work, where the strains have to be recorded from several stations on the test specimen, there appear to be three principal methods of instrumentation.

In one method, which is extremely popular in America, the strain gauge bridge circuits, usually 6, 12 or 15 in number, are energised by a simple valve oscillator. Separate power packs and amplifiers are provided for each strain gauge circuit, the amplified signal being transmitted to a galvanometer, usually of the D'Arsonval or bifilar types. D.C. current is necessary for the current supply to the power packs and for the lighting system of the galvanometer.

Various galvanometer designs have been produced by the William Miller Corporation, Sperry Company, Hathaway Company, Heiland Laboratory, and others in America. Similar instruments are now in the prototype stage in this country.

The Miller instrument, which is one of the most popular, was especially designed for the flight testing of aeroplanes, but is equally useful in any application in which a compact multi-channel oscillograph is required. The instrument is usually supplied with six or twelve galvanometers which make a record, in the case of the twelve channel unit, on

photographic recording paper six inches wide. The external dimensions of the twelve channel oscillograph are $9\frac{5}{8}$″ wide, 10″ high and 22″ long.

The galvanometers, which are mounted in a common magnet, are of unique design, each consisting of a D'Arsonval galvanometer of very small dimensions. The moving coil is wound on a bobbin machined from solid stock, the natural frequency of the element being fixed by the torsional stiffness of the suspension ribbon. Damping is provided by means of the back e.m.f. in the coil and eddy currents in the bobbin for the low frequency elements. For natural frequencies above about 200 c.p.s., the electro-magnetic damping becomes negligible compared to the stiffness of the suspensions and it is necessary to

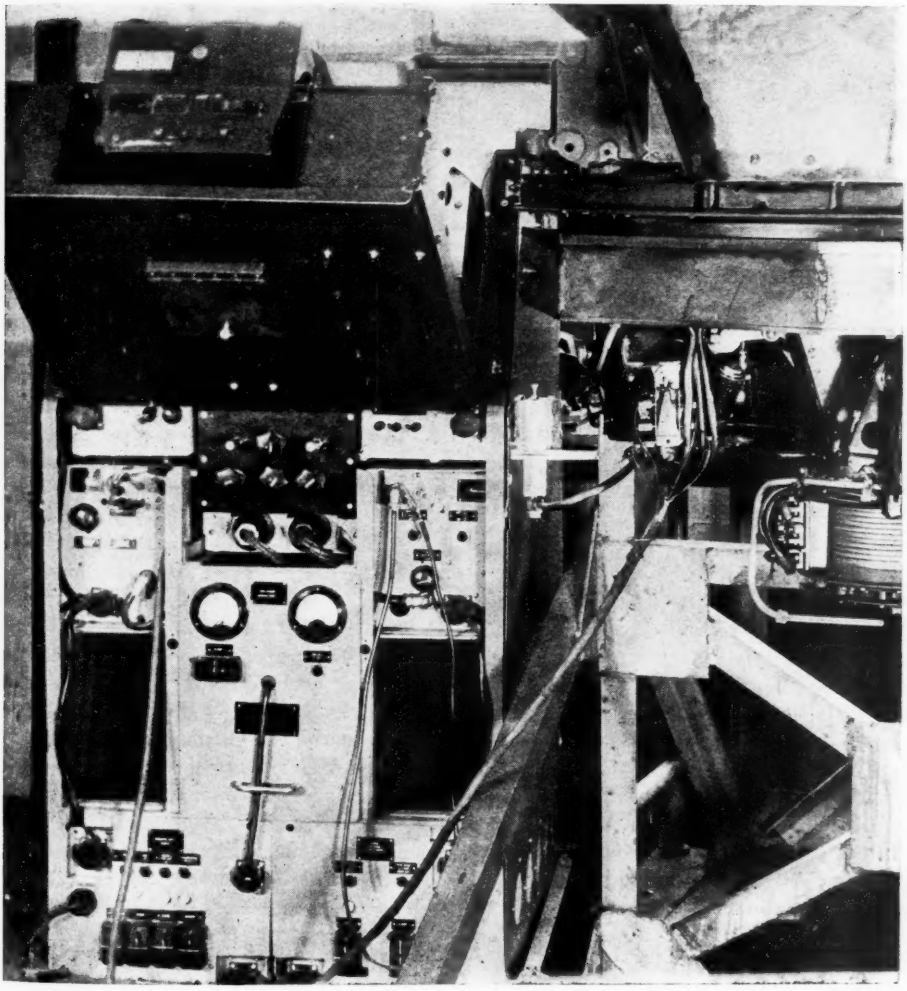
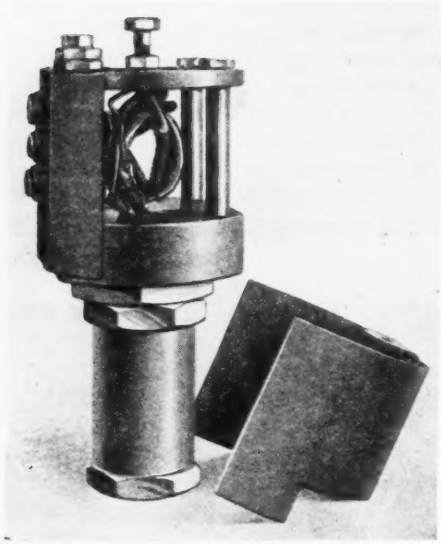


Fig. 26



PRESSURE UNIT

DISPLACEMENT UNIT

Fig. 27

use oil damping which is provided so that the galvanometer mirror is not covered by the damping oil.

Galvanometer elements of different natural frequencies are interchangeable and a very simple astigmatic optical system is used. The oscillograph concave mirrors are cylindrical in shape with their axes parallel to the axis of the galvanometer and a line filament lamp is used as a light source. Each mirror focusses an image of the filament at the paper distance, a cylindrical lens of short focal length being placed with its axis normal to the direction of the paper travel, and at such a distance from it as to focus the eight spots on the paper. Provision is made for photographic timing lines on the paper simultaneously with the galvanometer recordings. Provision is made for visual scanning, which can be done simultaneously with, or without, recording.

The necessary natural frequency of the recording galvanometer depends upon the frequency range over which a reasonably flat response is desired. The natural frequency of an undamped galvanometer of the D'Arsonval type is proportional to the square root of the torsional stiffness of the suspension and inversely proportional to the square root of the inertia of the suspended coil and mirror. If the coil size and weight is reduced to a minimum, the only way in which the natural frequency can be raised is by increasing the torsional stiffness of the suspension.

It is clearly desirable that the motion of the galvanometer recording trace shall conform as rapidly as possible to the signal which is being recorded. It is also necessary that increased response of the galvanometer near resonance be suppressed. For these reasons the galvanometer must be damped. In the Miller instrument this appears to have been accomplished effectively for the higher frequency range by the measure of oil damping provided. Several descriptions of this and similar instruments have appeared in the technical press.[10] [11]

[10] C. G. A. Woodford: Electrical Strain Gauging. Aircraft Production, May, 1944.

[11] C. M. Hathaway: Electrical Instruments for Strain Analysis. Experimental Stress Analysis, Vol. 1, No. 1, p. 83.

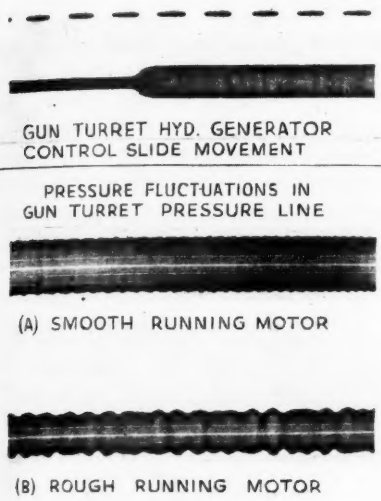


Fig. 28

A twelve channel D'Arsonval type galvanometer, which is being developed at Boulton Paul Aircraft, is shown with its amplifiers and power packs in Fig. 29. This instrument, which is now under development, has been designed so that the traces from the twelve galvanometers will be recorded, in sets of six, on two 70 mm. films. The motion of the two films is synchronised so that the twelve channels may be used simultaneously. Alternatively the set will operate as a six channel unit if required.

Another method is to use a battery of miniature cathode ray tubes, each having its own amplifier. The tubes are mounted close together, the display from them being photographed by a moving film camera. It is possible to obtain tubes as small as $1\frac{1}{2}$" in diameter and an excellent instrument of this type has been produced by the de Havilland Aircraft Company Ltd. As with the previous method, a simultaneous recording is obtained from all the strain gauges, but a limitation is placed on the number of channels which can be used by the space in which the tubes can be mounted for photographic purposes.

Six tubes can be incorporated in instruments of this type and it would seem to be

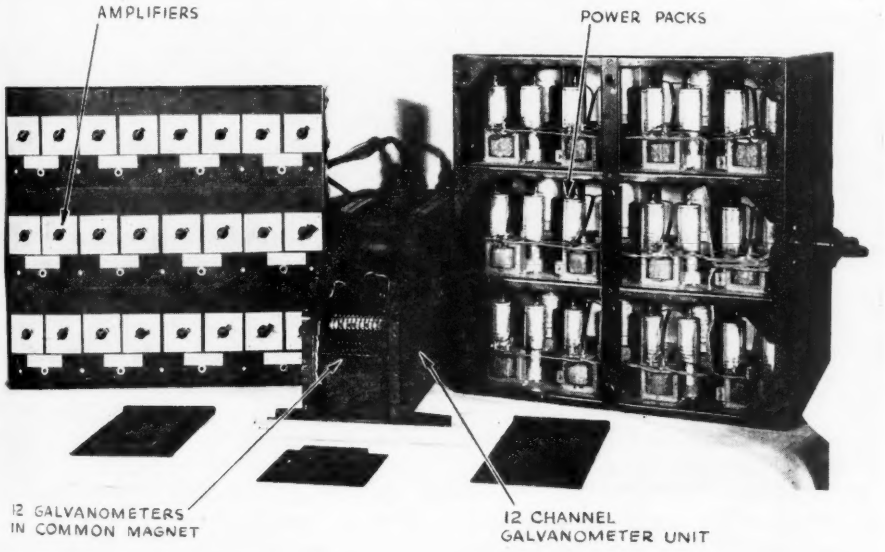


Fig. 29
Multi-Channel Galvanometer Recorder

possible to use up to twelve tubes by a suitable arrangement. A great advantage of this method lies in the fact that the functioning of the cathode ray tube is independent of the frequency of the impressed signal. The disadvantage lies in the fact that the spot on the tube is relatively large and the traces produced by it on the moving film are thick in comparison with the electro-magnetic galvanometer. With both types of instrument a time base is, of course, necessary and this can easily be produced by means of a mercury vapour discharge lamp operated by a master contactor switch.

A further method which can be used appears, at present, to be limited to two or four channels. In this case the signals, say from four strain gauges, are switched in turn on to the cathode ray tube for a small period of time, the switching being done by means of a chopping switch consisting of an electrical motor rotating at a high speed and operating a mechanical set of switches. As a result there are displayed on the tube four traces simultaneously. At the present time each of four channels can be switched in at about one hundred times per second, and this method is therefore unsuitable unless a record is being made of oscillatory strains which do not have a frequency of more than a few cycles per second. The rate of switch therefore imposes a serious limitation on this method, and for higher frequency work a possible solution would appear to lie in the use of some form of electronic switching. The Electronic Recorder previously described would be quite suitable for this mode of operation.

For multi-channel recordings of dynamic strain it would appear that the electro-magnetic galvanometer offers the best solution, and it is hoped that suitable galvanometer units will soon be available for use in this country. The compact size of the galvanometer renders it ideal for flight test work where space for instrument installation is often so very limited.

4.0. CONCLUSION.

In this paper I have attempted to outline the methods available for the electrical measurement of strain, and I hope I have caused no disappointment by showing such a marked preference for the electrical resistance method. I consider that the development of the multi-channel oscillograph is our most urgent problem in strain gauge work, if the really wonderful potentialities of the wire resistance gauge are to be fully explored.

It is only natural that the versatile electrical strain gauge should have been used, during the early stages in its development, in applications where a normal mechanical extensometer would have been just as good. Also, the initial tendency was to use several hundred gauges on a test specimen, thus providing the stress analyst with a vast mass of indigestible data. The rule should be, not "How many gauges can be cemented to a specimen?" but "From how small a number can an appreciation of the strain distribution be made?" Much work remains to be done on the selection of strain gauge stations for correct strain interpretation, and there is evidence of a growing realisation of this problem.

We are only at the beginning of electrical strain gauge development; the wire resistance method has been developed almost entirely in the past six or seven years, and the possibilities of the method appear to be limitless.

In conclusion, I should like to express my thanks to Mr. J. D. North for the great interest and encouragement he has always shown me in this work. My thanks are also due to my colleagues at Boulton Paul Aircraft Ltd., who have so enthusiastically assisted in the development of some of the methods described in this paper, and to the Ministry of Aircraft Production and Boulton Paul Aircraft Ltd., for permission to publish details of work done on their behalf.

DISCUSSION

MR. J. TAYLOR (Royal Aircraft Establishment, Associate Fellow): Dr. Redshaw had mentioned the use of a pair of gauges to measure direct strains, one gauge compensating the other. A pair of gauges could also be used in a similar way to measure shear, by placing the two gauges at right angles to one another and at 45° to the required direction of shear. The gauges were placed on opposite arms of a Wheatstone bridge and automatic temperature compensation was achieved. In making strain gauge installations at the Royal Aircraft Establishment, shear measurements had been found more reliable than direct load measurements, as they were not influenced so much by local effects of rivets and bolts.

The Establishment had a cathode ray tube and high speed switching set for static strain measurements, a 600 point set which used direct current to the gauges instead of a carrier wave system, and a complete set of readings was taken in 10 seconds. The staff had overcome the drawbacks which Dr. Redshaw had mentioned. They viewed the screen and photographed the results simultaneously, and at the same time a pointer showed whether the film was moving through the camera. The only possible source of trouble which could not be detected during the test was the development of the film, and this would very rarely be so bad that the strains could not be read.

Special precautions were taken to detect gauge failure during tests. Common busbar leads with switches were used for each set of 8 gauge circuits, so that should a gauge become inoperative it was possible to determine in 15 seconds to which eight it belonged and to cut out the group of eight for that reading. It would take another minute to locate and remove the offending gauge circuit, so that the other seven could be read at subsequent loadings.

He particularly endorsed Dr. Redshaw's remarks on the minimum number from which strain distribution could be appreciated. About 200 measuring positions had been found necessary to investigate a major point of design, but considerable care had to be taken to concentrate them at the correct places. Above all, it was necessary to be quite certain that the staff was available to do the subsequent analysis, before any installation was undertaken. The paper had covered a wide field in an admirable manner.

MR. D. H. PEIRSON (Royal Aircraft Establishment): Had Dr. Redshaw any information, other than that given in the lecture, on the strain sensitivity factor of the various types of wires described? Apparently the factor could be accounted for as far as 1.6—as far as the effects due to change in geometry of the gauge, which was one plus twice the Poisson ratio. Large improvements and considerable advances in materials for strain gauge wires would be possible if some fundamental work were initiated into pure strain sensitivity effect, other than the effect due to the change in geometry.

He agreed, from his experience of the use of wire resistance strain gauges in the open, with what Dr. Redshaw had said, especially on the overwhelming importance of providing an efficient weather proofing or water proofing agent. The staff of the Royal Aircraft Establishment had found in making flight tests that it was essential to coat and recoat the gauges even if they were only intended for use during a few days in the open. After considerable difficulty they had eventually arrived at an effective covering for the 200 ohm British Thermostat gauge, consisting of a layer of "Digel," which was a soft wax providing the initial water proofing; this in turn was covered with a form of "Bostik," which provided mechanical protection, and over it was applied a further coating of "Digel." Specimens had been exposed in winter storms and for a number of gauges it had been possible to maintain resistance to

earth (*i.e.* to the specimen) of over 100 megohms. This he considered a satisfactory result.

A comparison of resistance, capacity and inductance strain gauges seemed to him to favour the use of resistance gauges. The resistance gauge required no additional precircuit before amplification. This seemed an important advantage where a number of channels were in use and where it was important to reduce space occupied by the equipment. Capacity and inductance gauges usually required an additional precircuit to convert from capacity or inductance change to voltage change, whereas the resistance bridge did that conversion in the simplest manner.

In principle, he agreed with what Dr. Redshaw had said about direct current amplifiers, but he had had occasion to use, or to watch being used, the twelve-channel Miller accelerometer and strain gauge equipment. This incorporated carrier type amplifiers responding to zero frequency. The strain gauge bridges were supplied from an oscillator; amplification took place at the oscillator carrier frequency, and finally, before recording, the modulated signal was rectified. That amplifier appeared to give very good stability; he had been surprised at its steadiness over periods of one or two hours.

He asked why, in the multi-channel method of measuring static strains with an alternating current amplifier, a carrier wave had been used with a form of switching. Using a direct current bridge supply, if switching from one position to another was fast enough, it was possible in effect to obtain an alternating current signal and suitable and accurate response from a conventional alternating current amplifier.

He defended the method of high speed switching generally, and maintained that it was possible, by passing a number of channels through a single amplifier to a single indicating instrument, such as a cathode ray oscillograph, to get an overall calibration in one operation, whereby the amplifier and C.R. tube could be calibrated in terms of all channels. When using separate channels and indicating instruments it was necessary to carry out the more tedious separate calibrations, the labour increasing with the number of channels. Also, if the switching was arranged to include a base line, by deflecting the C.R. trace to a zero reference between scanning each channel, a check was provided on the behaviour of the electronic part, as distinct from the gauge part of the equipment. If the whole equipment were subjected to vibration or other shocks in the course of the measurement—a normal occurrence in flight—observation of a constant and steady base line confirmed that the amplifier and so forth were free from microphony and other undesirable effects. Thus it was possible to ensure that the equipment was not making spurious contributions to the signal that was being measured.

MR. J. G. STRONG (Associate Fellow): They had had about six years' experience of the development of strain gauges from the beginning of the war and had seen great changes in the electronic part of the equipment. He had been somewhat disappointed to find that the same amount of improvement had not been shown in the development of the strain gauge itself. The limit of most gauges was a strain of about 5,000 micro-inches per inch, and many gauges ceased to measure before that point was reached. With 5,000 micro-inches strain and a modulus of elasticity for dural of 10, the stress was 50,000 lb. or about 20 tons per square inch, as the limit of the gauge. Nowadays aircraft engineers worked to 35 or even to 38 tons per square inch, and so were unable to measure up to the ultimate strength of the material.

The same restriction applied to steel, where the limit was about 65 tons, but the actual stresses in practice could reach 75 or 85 tons sq. in. In measuring the

strain on wood the limit of stress was 5,000 or 6,000 lb. to the square inch, whereas the actual stresses might be as high as 10,000 lb. to the square inch. Could Dr. Redshaw hold out any hope of improvement, especially in the elasticity of the gauge, so that it would be possible to measure these higher strains?

MR. D. A. R. CLARK (Associate Fellow, Chairman, Luton Branch of the Society): As a newcomer to the use of strain gauges, he would like Dr. Redshaw's advice on the best resistance to use. One of approximately 2,000 or 2,500 ohms had been recommended. Also, what was the best voltage?

He had not followed Dr. Redshaw's account of the method of fixing the acoustic strain gauge to the structure. Had any means been devised of showing the vibrations on a screen, rather than trying to match their note by ear? He had tried some years ago, in measuring the strain on flat plates, to match the note emitted when a flat plate was tapped to the note of a sonometer, and had found it exceptionally difficult. If the vibrations could be thrown on to an oscillograph they could be matched visually and, he would have thought, much more accurately.

MR. P. J. HIGGS (Engineering Department, National Physical Laboratory): If a transient current passed throuph a circuit which was in any sense wound inductively, inductive voltages would occur across certain points. He presumed that workers with these gauges in various electrical circuits were fully alive to this phenomenon and took every care that their resistances and other coils were wound absolutely non-inductively; otherwise under transient conditions they would have errors in measurement. These gauges were used on aircraft in flight, and he visualised that, at the high speeds that would soon be reached, errors would be introduced owing to the rapid passage of the gauge through the earth's magnetic field. Was this a possibility?

In the testing of engineering materials it was common practice to measure both strain and load, and he felt that to measure strain alone was only part of the story: attention should be given to the measurement of loads at the same time. Complete records of load and strain phenomena under all conditions would indicate exactly what was happening.

MR. J. B. B. OWEN (Royal Aircraft Establishment, Associate Fellow): He also emphasised the need for protecting strain gauges against moisture changes. The change, and not the actual presence of moisture, was the important factor, unless the moisture happened to be very copious and initiated some sort of electrolysis. It was necessary to know when making a test whether any hysteresis was due to the structure, or whether the apparent hysteresis came from a strain gauge drift due, either to bad temperature compensation or to bad moisture protection, allowing the strain gauge to change its moisture content and probably expand, as well as change its earth resistance.

Another point on which workers might go astray if they pushed it too far was the particular formula which indicated that the resistance of the gauges should be raised as much as possible when a cathode ray tube was in the circuit. When switching was used to record several signals on the same cathode ray tube, this in present practice was shunted by a high resistance. The simplified formula quoted would break down as gauge resistances became comparable with this resistance.

Dr. Redshaw's presentation of his paper must have made the subject clear to many engineers who had little knowledge of electronics or electricity.

MR. K. W. NORRIS (Student): He had three questions on the use of resistance wire strain gauges: 1. What was the effect of the thickness of the layer of cement between the gauge and the specimen on the effectiveness

of the gauge? 2. What was the pressure required to secure the gauge satisfactorily to the specimen, particularly the British Thermostat gauge? He had experienced difficulty in applying sufficient pressure to squeeze surplus cement from under the British Thermostat gauge in order to obtain close and uniform adherence. 3. The British Thermostat gauge already had cement incorporated in the paper backing and had to be soaked in acetone before application to the specimen; would not the acetone adversely affect the original cementing of wire to paper?

MR. K. C. PRATT (Associate Fellow): They had been given some values for the strain sensitivity of the gauges but no indication of the variation which might be expected within one batch of gauges. He understood that Baldwin and Southwark said one per cent and other makers said 2 per cent. In his experience it was something like 3 or 4 per cent. Had Dr. Redshaw any figures?

DR. AUGHTIE (Engineering Division, National Physical Laboratory. *Contributed*): He had read the paper with considerable interest as he had had experience of various electrical remote recording devices extending over twenty years. He thought that there were three essential factors which were responsible for their extensive use: (1) that air was a non-conductor; (2) that the velocity of propagation of electrical waves was about five thousand times that of mechanical stress waves; and (3) the possibility of considerable amplification.

As regards the relative merits of the different methods mentioned in the present paper, he had no personal experience of either acoustic or piezo methods. As between the others he agreed that for measurements of strain upon a structure, the resistance strain gauge had such an outstanding advantage that it certainly warranted first consideration. But the amount of output power from a gauge (and associated bridge circuit) for ordinary values of strain was small and was much less than for an inductance method working with comparable displacement. For instance, if a 2,500 ohm gauge carrying 20 mA (a high current) was strained by 0.0001 its resistance change was about 0.5 ohm so that the "open circuit" EMF arising from the strain was 10 mV. For optimum sensitivity one half of this would appear across a galvanometer of 2,500 ohms resistance, giving a galvanometer power of current of 5/2,500 mA and a galvanometer power of 1/100 micro watt. An inductance device which he had designed (but which was of course very much more elaborate) gave, for a displacement of 0.0001 in., an output current of nearly 2 mA through 30 ohms or 100 micro watts. Thus where *e.g.* a load gauge was required and the constructional complexity could be tolerated, an inductance device should be considered.

The variable capacity device, in general, was intermediate between the other two but with sound design and workmanship seemed to offer good hope of much greater precision than either and appeared to be best suited to problems of standardisation. Its other advantage, that its upper frequency limit as determined by purely electrical considerations was much higher than the other two was, in practice, largely discounted by the fact that, save in specially favourable cases, the upper limit was always determined by mechanical factors.

In the author's treatment of resistance gauges there were a number of points with which he could not agree, and others where further detail was desirable. It was believed that the first gauges "moulded between layers of bakelised paper" were made at the National Physical Laboratory in 1941 and that any development and manufacture elsewhere was appreciably later and followed from that work. The N.P.L. had made gauges down to 0.05 in. in length and had moulded them to a "saddle-back" shape; they had also made triple or rosette gauges by this method, as well as many "double"

or "crossed" gauges specifically for shear measurements. Nichrome wire had been used initially because it was available. The N.P.L. had spot welded the leads to the wire from the beginning. For about the past two years they had been using leads of nickel-copper alloy (with nichrome wire); this greatly simplified the soldering of wires to the gauge leads but admittedly increased somewhat the risk of error from thermo-EMF's.

The author, in discussing temperature coefficient, omitted any reference to differential strain of the wire of the gauge arising from inequality of linear coefficients of expansion of gauge wire and specimen and thereby might lead users to think that the resistance coefficient was the only relevant factor. He wrote of placing the compensating gauge near the main gauge; this might suffice in some work but it seemed desirable to emphasise that the essential requirement was of equality of temperature. Personally, he disliked the phrase "apparent change in strain," when, in fact, the gauge suffered a real change of resistance.

N.P.L. practice with regard to glueing was to pre-coat both gauge and test surface: they always baked on the gauges whenever possible. If baking was not possible twenty-four hours was far too short a time for reliable results with bakelised paper gauges.

In the pre-print the author derived the equal arm bridge. This popular fallacy seemed irresistible to nearly all workers and seemed to date back to the time when primary batteries of appreciable internal resistance were the only source of power. There was no error in the author's mathematics; his mistake was in assuming that $(V_W - V_E)$ should remain constant as P was increased. Elsewhere he implied, correctly, that gauge current was the essentially constant factor and when this was inserted the optimum condition became $P=\infty$. In practice $P=10R$ gave only slightly less sensitivity than this unattainable optimum.

He had not found that the (signal/noise) ratio* deteriorated with increase of gauge resistance and, in fact, if the pick up of noise was entirely from "electrostatic" fields this ratio should be independent of resistance for a constant gauge current. The objection to high gauge resistances (for dynamic work) was the loss of high frequency components owing to the shunting effect of the cable capacity.

The author did not seem to be quite clear on the question of absolute value of gauge resistance. The writer had discussed this elsewhere.† Briefly, the problem was as follows. All indicating or recording equipment applicable to strain gauge work required power to give a deflection. (This power was that from the gauge circuit directly and was irrespective of auxiliary supplies to amplifiers, etc.). Instruments differed only in their resistance, and it could lead to false reasoning to speak of a galvanometer requiring "current" and an amplifier requiring "voltage." Optimum sensitivity occurred when the resistance of the indicating device was matched to the effective output impedance of the gauge circuit. With a galvanometer, there was usually a choice of resistance for instruments otherwise similar; there was then an equivalent choice of gauge resistance for optimum sensitivity. In this case the gauge resistance could be chosen by reference to secondary factors. These were: (1) For a given size of wire (a practical restriction) then, provided the winding pitch was not too small, the output power from the gauge circuit increased linearly with gauge resistance; (2) Errors due to thermo-EMF's, resistance of leads and variation thereof decreased with increase of gauge resistance; (3) Errors due to insulation resistance (leakage) increased with increase of gauge resistance. In practice, using a galvanometer, (3) was usually

* "Noise" was a convenient term to include all spurious voltages in the circuit due to external causes.

† See Proc.I.Mech.E. Vol. 152, p. 240.

the determining factor. With an oscillograph the input resistance of the associated amplifier was usually much higher than could be matched by practicable values of gauge resistance; the highest attainable gauge resistance was then the best. But if long cables were used and good high frequency response was required, it was necessary to match the gauge to the cable impedance and much lower values of gauge resistance became necessary.

The author's circuit of Fig. 12 as shown could be improved by interchanging the gauge and the large resistor; in this way one end of the gauge would be placed at earth potential. In Fig. 13 and the associated description there was no reference to the need for quadrature balance of the bridge. This, together with the author's reference to adjusting to a "minimum value" and his remark that "tensile and compressive strains are indistinguishable" suggested that the author did not provide for such quadrature balance. But if the bridge was so balanced (as by the provision of a variable condenser across one of the arms) an exact null would be obtained (with a sinusoidal supply) and if the bridge was then thrown into resistive unbalance, tensile, and compressive strains were easily distinguished. If the bridge was not in quadrature balance then not only did the sign of the strain become indistinguishable* but the overall calibration from gauge to oscillograph became non-linear, the extent of the non-linearity varying with the amount of resistive unbalance, and the system was virtually unusable.

For calibration of the gauges themselves, the N.P.L. did not use a compensating gauge but a resistance box and took intermediate zero readings to guard against temperature change. The author suggested placing a shunt across the gauge as a convenient way of avoiding the need for small changes of series resistance. This was sound; but if one P-arm was shunted and initial balance was secured by varying the other P-arm, calibration could be done without a separate determination of gauge resistance.

He must disclaim for the National Physical Laboratory the original suggestion of photographing a bank of galvanometers: they derived this from the Royal Aircraft Establishment. They had however developed a particularly convenient and accurate way of calibrating such equipment.† They had also developed protecting circuits which permitted the galvanometers to be left in circuit during tests to destruction.§ On the question of multichannel recording, the author condemned one method on the grounds, *inter alia*, that the C.R. tube could not be seen or that the camera might fail. A second tube in parallel, or a peep hole, removed the former objection and the latter applied equally to all methods. The real objection seemed to be that since the channels were commutated rapidly, the eye could not select any one for observation. But for visual checking either a synchronised stroboscope, or better, synchronised beam switching of the second tube, would permit individual examination of the channels. A more important problem was that of " cross-talk " or the influence of one channel on another. This problem was fully understood since it arose in multiplex communication circuits. Methods of commutating at high speeds (order 1,000 complete cycles/sec.) were known and a special tube had been developed in the U.S.A. which would switch 30 channels.

In conclusion he wished to express his appreciation of the paper as a whole and he hoped that where he went beyond differences based solely on personal preference his remarks would be regarded as constructive criticism.

* Except by reference to the relative phase of the output.

† See the writer's paper "Electrical Resistance Strain Gauges . . ." read before the Institute of Marine Engineers, March 16, 1946.

§ See Pat. No. 19865/43.

MR. REDSHAW'S REPLY TO THE DISCUSSION

MR. TAYLOR: His method of using two gauges at right angles and at 45° to the required direction of shear was a useful method only if the principal axis of strain were known. It was important to realise that for the complete determination of strain at a point either three strains must be determined, or two strains and the orientation of the principal axis must be known. It would not be sufficient to place two gauges at 45° to a direction along which a shear strain was required.

Much valuable work was being done at Farnborough on the correct position in which gauges should be used. In the early days enthusiastic workers were apt to apply gauges all over the structure "like a set of postage stamps," and hand the result to the stress office and tell them to get out the answer by lunch time. He thought it was better to use a few gauges only and analyse their results properly.

MR. PEIRSON: He had asked what improvements could be obtained in raising the sensitivity factor of the wires in use. A wide field of research could be opened into the selection of wires for strain gauge work. It was true that the sensitivity factor apparently depended on Poisson's ratio and, of course, on the change in resistivity when the wire was strained. It was not certain that Poisson's ratio held good for fine wires. Moreover, there was little evidence of the amount of change in resistivity when the wire was strained; it seemed practically impossible to forecast the strain sensitivity factor of any new wire which was introduced. He thought that the only way to proceed was to take wires and test them; this was research work which some metallurgist might usefully do. Obviously, if the strain sensitivity factor could be increased, the problem of amplifying the signals would be to a large extent solved.

A wide variety of methods were used for weather proofing and protecting the gauge. Normally his staff coated them only with a very heavy layer of vaseline, but for flight work it was necessary to take other precautions.

He was always open to conversion to the merits of Direct Current Amplifiers but had been very nervous about their use. It was necessary first to stabilise the source of current, then to transform, rectify and smooth it. There seemed to be many things that could go wrong with that type of amplifier. He had, however, no doubt that with the great attention which was now being paid to amplification for strain gauge work, a really reliable direct current amplifier would shortly be provided.

In asking why a carrier wave was used when switching was incorporated, Mr. Peirson had almost provided the answer himself by making the proviso that the switching should be fast enough. The reason was that the switching was probably not sufficiently fast. Present day switching had been practically limited to mechanical switching; some work was being done in the official establishments on electronic switching and if this could be developed to a practical point, a satisfactory solution to the problem might be reached.

MR. STRONG: The figure he had given as the limit of the strain gauge had been $5{,}000 \times 10^{-6}$ strain, and Mr. Strong had endorsed this figure, which he thought was very nearly an outside limit, and only gave a corresponding stress in duralumin of 22 tons per square inch, and 66 tons per square inch in steel. He agreed that this certainly needed to be raised if possible. This was another problem demanding research into wires, and more particularly into the method of bonding the wire into the strain gauge itself. The properties of the wire were not the only factor, the characteristics of the cement and paper which were an integral part of the gauge were also important. The problem of increasing the range would be very difficult

to solve and he had not yet seen any hopeful signs of its solution.

MR. CLARK: A high resistance strain gauge was preferable for use on dynamic work with a cathode ray tube, but he did not think that a resistance greater than 2,500 ohms was necessary. If the resistance of the gauge was increased more wire had to be put in the gauge, and the consequent increase in its size was undesirable. There was also the danger of the resistance breaking down through the insulation failure of the paper and cement to which the gauge was attached. For most purposes he had found that a 200 ohm gauge gave very satisfactory results. It could be used for either static or dynamic work without much loss of sensitivity. Although wire resistance gauges with a resistance as low as 50 ohms could be obtained, he did not think they were worth using. He had tried a 50 ohm gauge and not found it very satisfactory. The Americans had seemed to favour 120 ohms, and produced many gauges between that and 350 ohms, but for all general purposes he thought that a 200 ohm unit would give what was required. The correct voltage to use depended on the resistance of the gauge and the limit which was to be set to the current passing through it.

He regretted that he had not shown the method of fixing the acoustic strain gauge. The diagram showed the gauge without its case and lying on its back. The two knife edges were placed on the structure and the gauge was then held down by a stirrup fitted at either end with a spring which was hooked round a screw or a cup hook inserted in the structure. He sympathised with Mr. Clark who had thought it might be difficult to match the notes from the acoustic gauge, and confessed that he had never been able to do this himself. He believed that the vibrations had been displayed on a cathode ray tube, particularly where the gauge had been used for dynamic work, where the observer obviously had not time to match the notes himself. The method proved, however, to be very laborious.

In calibrating strain gauges he had been in the habit of testing one matched pair from each consignment of gauges. The makers supplied the gauges in packets, and all the gauges in one packet were made from one batch of wire under the same conditions. He accepted the calibration for one pair of gauges as holding for the rest of that batch. A gauge was, of course, useless after it had been attached to the calibrating beam, for it could not be detached. He strongly maintained that if it could be detached it had not been secured properly in the first place. If it was properly stuck down with durofix it could not, short of scraping it off with a knife, be unstuck. One of the disadvantages of the wire resistance gauge was that it could not be taken off and given another test; once it had been attached the answer it gave must be accepted. Mechanical gauges were much easier to calibrate.

MR. HIGGS: On the question of whether the strain gauge returned to zero, he would remind them that the specimen itself possibly contained some hysteresis. A non-return might be due to slight overstrain of the specimen. Gauges would fail to return to zero if they had been overstrained themselves or if there had been any non-compensated temperature or moisture change during the experiment. Another cause of drift of this sort was overheating of the wire of the strain gauge by passing too high a current through it. He recommended 20 ma. for the nichrome wire gauge of 0.001 inch diameter wire as the absolute maximum. The current passing through the gauge should be kept as low as possible.

He appreciated that for transients non-inductive winding was necessary, but he thought that the method of manufacture of the gauge, particularly if the zigzag drawn method was used, eliminated this difficulty and it should be remembered that if a compensating gauge was used any small change

due to non-inductive winding would be cancelled out.

MR. J. B. B. OWEN: He agreed that the change in moisture and not the actual amount of moisture was what counted, and the same applied equally to temperature. Test conditions in America were different from those in this country, for the climate was much drier. The relative humidity in Wolverhampton, for example, was nearly always over 80 per cent throughout the year. He suggested that all tests, especially if they were not of short duration, should be done if possible at night as far more stabilised conditions were likely to be obtained than during the day. This was easy for bridge testing but might be more difficult in flight test work. Strains could only be transmitted to the gauge by the shearing action of the paper and cement lying between the gauge wire and the surface of the specimen. It was therefore important that the layer of the cement should be as thin as possible and all the air bubbles trapped between the gauge and specimen should be pressed out. A thick layer of cement would give an apparent lowering of the gauge sensitivity factor. A phenol-formaldehyde resin was used in the British Thermostat gauge for bonding the wire to the paper backing. This resin, which was cured under heat and pressure, was impervious to the action of acetone which merely softened a cement, having an acetone base, which was placed on one outer surface of the gauge. The immersion of the gauge in acetone was very desirable as it softened the outer cement and rendered the gauge supple.

There was no danger of using a gauge having a resistance approaching the resistance of the cathode ray tube which was very high. He would not advocate the use of any gauge with a resistance of more than 2,500 ohms, because of the possibility of a breakdown in the insulation of the gauge paper and cement.

MR. NORRIS: The effect of the thickness of the layer of cement was very important. With regard to the effect of pressure on the gauge when it was stuck on, Baldwin-Southwark recommended that the gauge should be pressed on to the specimen, first by hand with a load not exceeding about 3 4 lb. and then clamped in position. His staff had never actually done that, but had only pressed the gauge on, placing a finger in the centre of the gauge and rolling towards either end. If a hard roller was used, there was great danger of breaking the leads, or even of fracturing the thin gauge wire.

MR. PRATT: He was unable to give a definite answer about the variations of the strain gauge factor. He had not yet had the courage to test a complete batch of gauges but he had an idea that the Royal Aircraft Establishment had been doing some statistical work on this question. He admitted that he accepted the factor as determined from a pair of gauges from each batch delivered but he did not think the change was very great. He had found that, generally speaking, the wound type of gauge seemed to give a more consistent strain gauge factor than the drawn-type principally, he thought, because with the drawn type it was impossible to stick the whole of the wire down at once, as the loops had to be left free in order to disengage the needles and stick down afterwards. Of course, there could be a difference from gauge to gauge in the form taken by the adhesion of the loops.

DR. AUGHTIE: He was incorrect in his belief that the first gauges "moulded between layers of bakelised paper" were made at the National Physical Laboratory in 1941. De Forest and Leaderman* in a report published in 1940 stated, "Two simple methods of mounting the fine wires have been developed. In one, the wire is moulded between layers of bakelised paper."

It was unfortunate that Dr. Aughtie was unable to be present at the lecture, because

* A. V. de Forest and H. Leaderman: The Development of Electrical Strain Gauges. N.A.C.A. Tech.Note, No. 744 p. 3.

the remarks regarding the equal arm bridge on page 13 and Fig. 11 of the unrevised proof to which Dr. Aughtie referred, had been deleted and the new Fig. 11 now appearing in the paper was shown and discussed. No reference had been made in the paper to " . . . a galvanometer requiring current and an amplifier requiring voltage." Indicating or recording equipment for strain gauge work naturally required power to give a deflection; in the case of the galvanometer this was obtained by means of a large current at a small voltage, the resistance of the galvanometer being low, while in the case of the cathode ray oscilloscope a small current at a high voltage was used, the resistance of the oscilloscope being very large.

Dr. Aughtie did not explain why the circuit of Fig. 12 could be improved by interchanging the gauge and large resistor. The circuit described had been frequently used and had given good results.

No reference was made to the need for quadrature balance of the bridge circuit described in Fig. 13 because a capacity balance was not necessary for the operation of this simple system. Without the provision of a variable condenser the amplitude of the carrier wave could not be reduced to a very small amount. Tensile or compressive strains were indicated by an increase in amplitude and were, of course, indistinguishable, the increase in amplitude being proportional to the applied strain. This system had been used successfully. The use of a variable condenser, as suggested by Dr. Aughtie, was merely a refinement of the circuit and enabled the carrier wave amplitude to be reduced to a very small amount from the minimum which could be obtained with resistive balance, thus making a larger portion of the tube screen available for strain recording.

In common with the simplified system tensile and compressive strains would be indistinguishable and could not be discriminated, as suggested by Dr. Aughtie, unless the bridge was initially off-balanced and the amplitude of the balance did not exceed the amplitude change due to a change in strain. This method could be used irrespective of the incorporation of a condenser in the bridge network.

THE PRESIDENT: It remained only to pass a hearty vote of thanks to the lecturer for his paper and for the able manner in which he had answered the questions. It had been very interesting to have a paper on this subject, and he had always been fascinated by the possibility, using the strain gauge as a magic eye, of seeing what was going on inside the metal under load or in an aircraft in actual flight. Only a few years ago such a thing would have seemed incredible.

THE ROYAL AERONAUTICAL SOCIETY

Paper received June, 1946

THE EXPANDING DOMAIN OF AEROELASTICITY

by

A. R. COLLAR, M.A., D.Sc., F.R.Ae.S.

Professor Collar took his degree at Cambridge with honours in mathematics and physics; he also took an honours degree in physics at University of London. In 1929 *he joined the staff of the Aerodynamics Department of the National Physical Laboratory and was for some time Secretary of the Airscrew Panel and the Fluid Motion Panel of the Aeronautical Research Committee. At the beginning of the war he was specially attached to the staff of the Royal Aircraft Establishment, Farnborough, to take charge of a special group investigating aeroplane flutter and vibration problems. In* 1945 *he was appointed Sir George White Professor of Aeronautical Engineering in the University of Bristol. He was elected a Fellow of the Society in March,* 1944.

SUMMARY.

BY MEANS of a broad survey of the developments in aeroelastic science during the past ten years and of comparable developments in associated fields, in so far as they involve elastic deformation, the present paper attempts to show how the individual subjects concerned are developing strong interconnections. At one time the subjects were for the most part regarded as discrete entities; now they are developing into component parts of an integrated whole —the dynamics of a deformable aeroplane.

It is suggested that in the future it may be necessary to discontinue the present methods of treatment of the component subjects as separate, and to adopt a standard method of attack on the problem treated as a unified whole, especially when adequate computational aids become available. A modified approach, involving an arrangement of the subjects in a frequency spectrum, is also discussed. Some speculations on the elastic provisions which may be necessary in the future are included.

1. INTRODUCTION.

The branch of aeronautical science which has become known as aeroelasticity is of comparatively recent development. It was first studied as such rather more than a decade ago, when Roxbee Cox and Pugsley, following their pioneer work on loss and reversal of aileron control and on the rolling power of a monoplane, began a correlation of this work with the flutter investigations of Frazer, Duncan and Collar. This correlation led to the formulation and development of stiffness criteria for aircraft wings and components: criteria which have combined an admirable simplicity with a remarkable degree of success in the prevention of troubles involving elastic deformation of aircraft.

In 1937, Pugsley presented to the Royal Aeronautical Society a statement[1] of the progress of the subject to that date. Since then, no general review has been made, in spite of great developments and changes of emphasis, particularly during the past few years. The rapid advances in aeronautical science which have been made during the war have resulted in corresponding strides in aeroelasticity, and

the subject has been expanded to such an extent that it is now encroaching on fields which have previously been regarded as the preserves of other subjects; and indeed, the subjects involved may now be regarded as beginning to merge into one another.

The implications of this trend of development have an obvious interest, and it is thought that a discussion of the existing state of affairs may be helpful at the present time. Accordingly, the following notes have been prepared to indicate how aeroelasticity is beginning to link up with other subjects, and what may be the future methods of dealing with this coalescence of subjects.

2. THE ORIGINAL SCOPE OF AEROELASTICITY.

Although Pugsley's paper[1] drew attention to the possible extension of aeroelastic studies to tailplanes, in fact the phenomena dealt with referred exclusively to wings: and indeed, at the time, little attention had been paid to aircraft components other than wings. Work on tail units had been carried out in flutter and buffeting investigations, but from the point of view of stiffness wings presented the main problem. Pugsley illustrated his subject by a stiffness diagram in which the ordinate was a modified (non-dimensional) wing torsional stiffness criterion and the abscissa, nominally, the wing density: in point of fact his independent variable might equally well have been taken as time, since there had been a steady increase in wing density through the years, and he was regarding the future as a period of very high wing density.

He showed how the criterion had changed with increasing wing density, and how it had been modified by the influence of various phenomena in turn: these phenomena, in chronological order of importance, were:—

(*a*) Wing-aileron flutter.
(*b*) Reversal of aileron control.
(*c*) Wing flexure-torsion flutter.
(*d*) Wing divergence.

In the case of wing-aileron flutter, aileron mass-balance had provided the cure; for the other three problems, however, wing torsional stiffness was shown to be the property of prime importance. Pugsley's paper concluded with some speculations as to which of the three phenomena, if any, would be the determining factor for the elastic properties of wings.

3. THE FORCES INVOLVED.

As a preliminary to the discussion of the expansion of the domain of aeroelasticity, it is convenient here to consider what forces are involved in the subject. The name itself defines by implication two of the forces, namely, aerodynamic forces and elastic forces; and these appear alone in studies of unaccelerated motion such as problems of steady rolling or aileron reversal. The flutter and buffeting fields, however, introduce a third and equally important type, namely, inertia forces. In addition, there may be certain other forces, for example, gravitational forces which make contributions to the total stiffnesses; but these are relatively unimportant, and we shall restrict ourselves to the consideration of aerodynamic, elastic and inertia forces.

4. THE "TRIANGLE OF FORCES" AND PROBLEMS AKIN TO AEROELASTICITY.

It is illuminating to consider some of the problems governed by the three types of force mentioned above; to do this, we may make use of the diagram shown in Fig. 1.

The three types of force are placed separately at the vertices of the equilateral "triangle of forces" AEI, where the initials indicate the force concerned. We now write down the subjects originally within the domain of aeroelasticity, bonding them to the appropriate vertices as shown, and placing them within the triangle when they are

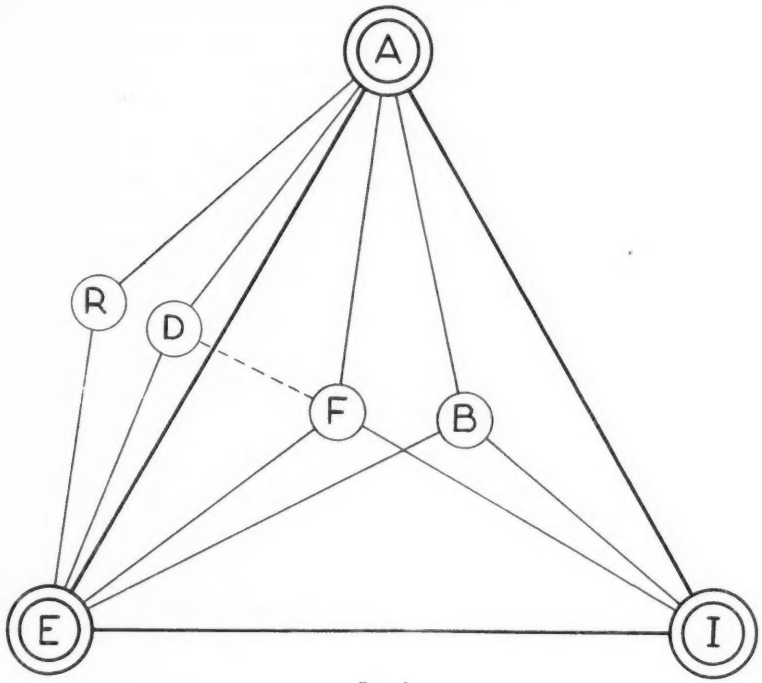


Fig. 1

A : aerodynamic forces
E : elastic forces
I : inertia forces
F : flutter
B : buffeting
D : divergence
R : reversal of control

bonded to all three vertices. Thus flutter (F) and buffeting (B) are located within the triangle. On the other hand, reversal of control (R) and divergence (D) are outside the triangle, since they are unconnected with inertia forces;* though we may draw a bond between divergence and flutter.

Inspection of Fig. 1, however, leads naturally to the question: in view of their obvious connections with aeroelasticity, what of the subjects in the aerodynamic-inertia force field and in the inertia-elastic force field? We may in fact add to our diagram, as in Fig. 2, a number of subjects originally belonging to these fields. In the first field is the vast subject of rigid aeroplane dynamics, of which a major component is the subject of stability and control (S); again, most loading or strength problems (L) of past years, including gust problems (G), have been treated on the basis of interaction of aerodynamic and inertia forces. In the inertia-elastic field is a subject which, at the time of Pugsley's review, was rapidly growing in importance—that of mechanical vibration (V); and entering this field we find the problem of the stresses induced by impact loads (Z) which, originally treated as an inertia problem only, was acquiring a connection with elastic forces.

Fig. 2 shows clearly that the no-man's land surrounding the territory of aeroelasticity was of very small extent; and any attempt at expansion must involve intrusion into other fields of work.

* By divergence we imply here an infinitely slow divergence, such as occurs at a critical divergence speed.

In the next paragraph we attempt to define briefly the scope of the problems shown in Fig. 2 at the beginning of the past decade of development.

5. DELIMITATION OF THE ORIGINAL SCOPE OF THE PROBLEMS.

If we were to attempt here to define any precise limits to the subjects with which we are concerned, we should have to include in a single paragraph a very large part of the whole science of aerodynamics. No apology is needed therefore if we deal with the subjects in a very broad and general way, and if in so doing we find that in our statement considerable omissions and lacunæ appear.

5·1. AEROELASTICITY.

We have already indicated, by reference to Pugsley's work, what was the general scope of this subject. It related almost exclusively to aeroplane wings, and laid down, on a theoretical-cum-statistical basis, standards of torsional stiffness for wings: standards of flexural stiffness were not found to be

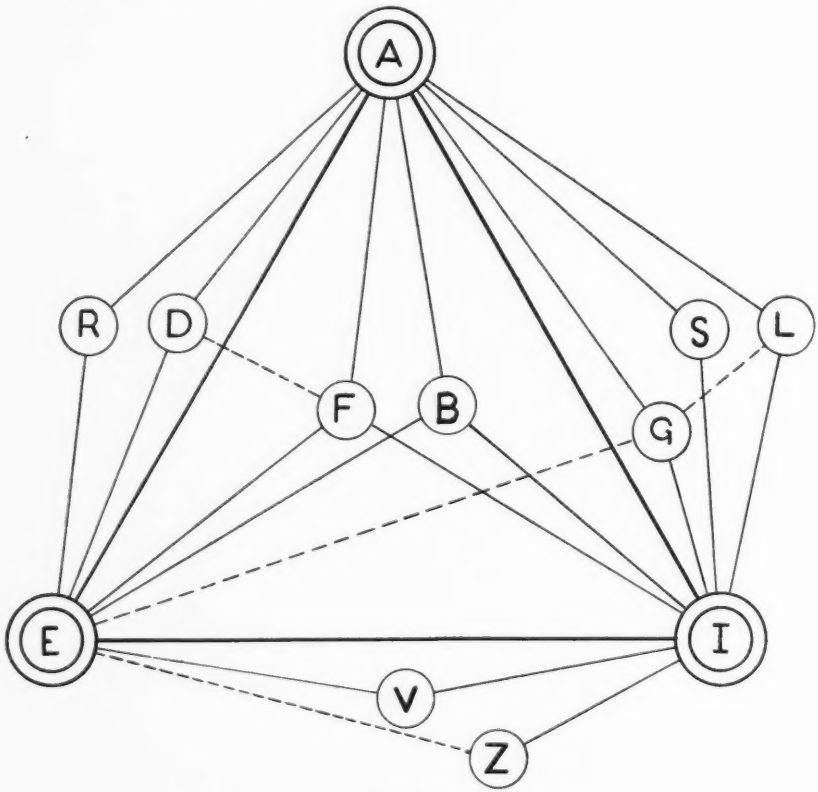


Fig. 2

A : aerodynamic forces
E : elastic forces
I : inertia forces

F : flutter
B : buffeting
S : stability and control
D : divergence
R : reversal of control
G : gusts
L : loading problems
V : mechanical vibration
Z : impacts

required. It is true that standards of torsional stiffness for ailerons had been tentatively proposed also; but the number of criteria was very small. In addition, the bases on which the component subjects were built up were quite narrowly limited in the manner indicated below.

5.1.1. *Flutter.*

The degree of complexity of the theoretical treatment of any subject by any one method increases very rapidly with the number of forces involved and particularly with the number of degrees of freedom which must be introduced. In consequence, flutter, which from the first involved all three types of force, and which was treated by the classical method of examination of the Routh stability criteria, had of necessity to be restricted to problems involving very few degrees of freedom: the maximum number was usually three. In addition, very considerable simplifications had to be introduced: of these, the most important was the concept of semi-rigidity, in accordance with which the elastic body with its infinite number of degrees of freedom is replaced by a body having only a finite number. For wings, these degrees of freedom were usually a single mode of displacement in torsion, another in flexure, and (rigid) aileron rotation.

On this basis, it was found that, up to ten years ago, most flutter problems could be adequately dealt with.[2] It thus appeared that it was unnecessary to introduce bodily freedoms of the aeroplane as a whole: this result was largely inferred from the satisfactory solutions obtained on the restrictive assumptions made, though one early research of Frazer and Duncan[3] had examined the effect of rolling freedom on antisymmetrical flexure-aileron flutter and had shown that little modification was required to the anti-flutter recommendations.

Broadly speaking, therefore, it was found to be possible to regard the fuselage—or at least the forward part—as rigid and infinitely massive, so that in effect it had no bodily freedoms; and the components whose flutter properties were to be studied were regarded as having only a small number of degrees of freedom and as being encastré in the fixed fuselage.

Even with the above simplifications, theoretical treatment was very complicated, and further assumptions were necessary, particularly for the formulation of a simple stiffness criterion. The aerodynamic actions, for example, were treated by strip theory, making use of a fundamental set of constant* derivative coefficients proposed by Duncan and Collar in a research on airscrew blade flutter.[5]

In the formulation of the stiffness criterion it was also necessary to treat of an "average" wing, in which the distribution of chord with span, the distribution of mass in the chord, and the position of the flexural axis, were all maintained constant: although in various researches the effects of these parameters were examined. Lastly, no distinction was made between the stiffness appropriate to the semi-rigid representation of a wing and the stiffness of the actual wing as found by test.

5.1.2. *Divergence.*

The problem of divergence can be treated either as a special case of the general flutter investigation or as a relatively simple problem *per se*. The remarks on flutter therefore apply also to divergence. There is, however, one additional limitation: a wing approaching its divergence speed, if encastré in such a manner that its root incidence were maintained constant, would change its lift with increasing rapidity. Strictly, therefore, a specification of the lift conditions, implying pitching of the fuselage, should be introduced; but this was generally neglected.

* It was of course realised that the aerodynamic forces are strictly functions of the frequency parameter: this dependence, for translational and pitching derivatives, had been investigated by Duncan and Collar.[4]

Some attention had been given to wing-aileron divergence; but the pilot's reaction to this phenomenon was generally ignored. At the wing-aileron divergence speed, both wing and aileron are neutrally stable, and the latter condition implies zero stick force. Thus a pilot will have warning, through the progressive lightening of his ailerons, of the approach of this phenomenon, and its critical speed can be controlled by alteration of the aileron hinge moment characteristics. Moreover, since aileron travel is limited, the corresponding wing twist is also limited: usually to an amount which would not imply danger to the structure. In view of these considerations, it is not surprising that wing-aileron divergence has not presented a problem of urgency in practice.

5.1.3. *Loss and Reversal of Control.*

Ten years ago this subject was restricted to loss and reversal of aileron control.[6, 7] Here also the remarks given above on flutter apply almost in their entirety; although some investigations into methods of dealing with aerodynamic loading[8] and into the case of an elastic (as distinct from semi-rigid) wing[9] were made.

In view of the relatively simple nature of the calculations involved, the results were not normally presented in the form of a stiffness criterion, although a torsional stiffness criterion of the normal form was involved; and hence parameters such as wing taper were taken into account in the calculations made.

5.1.4. *Buffeting.*

The subject of tail buffeting falls within the domain of aeroelasticity, but is in many ways a subject apart from the others. It is concerned[10] with the transient vibrations resulting in the tail unit from aerodynamic impulses due to the eddying wing wake. Since these impulses are quite random in character, there is no analytical theory associated with the phenomenon. Its cure is usually of an *ad hoc* nature; and by proper positioning of the tail organs and by clean design the phenomenon can be avoided. Since the whole trend of modern development has been towards cleaner designs, cases of occurrence of buffeting are now rare; and we shall not refer to the subject again.

5.2. Subjects Associated with Aeroelasticity in the "Triangle of Forces."

The subjects of 5.1 have been treated very broadly, and with an almost complete lack of detail. Even more broad and less detailed must be our attempt to delimit the scope, ten years ago, of the subjects with which we are now concerned. They had been studied extensively for many years, and were in consequence much more developed than aeroelasticity; we shall therefore only attempt to consider how they stood in relation to aeroelasticity.

5.2.1. *Dynamics of a Rigid Aeroplane—Stability and Control.*

The structure of this subject had been erected on a foundation of aerodynamic and inertia forces only: it had not been found necessary to introduce elastic deformations. It is true that the work of Gates showed an awareness of the possible importance of elasticity—he had at an early date been concerned with flutter as a problem in stability—but with the exception perhaps of control circuits, elasticity effects were generally ignored. The formal treatment of stability and control, including the separation of longitudinal stability from other aspects, was of course well established; and attention was therefore mainly directed to the important component details of the subject, and in particular to control surface hinge moments.

The development of aeroelasticity re-emphasised the question of elastic deformation; and in their work on the effect of response on aileron hinge moments, Gates

and Irving[11] introduced the appropriate aileron reversal speed correction.

It may be remarked that the exclusion of elastic forces was in fact quite natural, for two reasons: first, the frequencies of oscillation occurring in stability investigations were on the average much lower than the gravest natural frequency of elastic oscillation; secondly, there were very little data on the elastic properties of aircraft. Such information as existed was mainly of an *ad hoc* nature, and Roxbee Cox's statistical surveys were only just reaching a point at which it was possible, through the medium of criteria, to fix the desirable elastic properties of aircraft and to give guidance to the research worker.

5.2.2. *Loading Problems.*

In the determination of the aeroplane strength needed to resist the aerodynamic and inertia loads applied, elastic deformations were almost invariably ignored. For one thing, the number of loading cases to be covered, and the lack of precise information on the aerodynamic loads, implied margins of safety which were more than adequate to deal with variations in load due to elastic deformations. Again, for the main components affected, strength was largely determined by spar design, while stiffness provision was made (if the intrinsic spar stiffness was insufficient) by the addition of skin of such a gauge that modification to the calculation of the strength characteristics was unnecessary.

Thus, although it had been foreseen by Pugsley and Roxbee Cox[12] that ultimately there must be some inter-relation between strength and stiffness, that stage had not been reached; although in one field—that of gust effects—it was just being realised that structural elasticity must appreciably affect the magnitude of the loads due to gusts.

5.2.3. *Gust Problems.*

At the period we are considering, studies of gust effects were not very advanced, having been severely hampered by an almost complete lack of knowledge of the structure of gusts. But investigations of the loads due to gusts of arbitrary structure were being made under the guidance of Bryant[13] (for wing loads) and Williams[14] (principally for tail loads).

In the main these investigations related to rigid aeroplanes, but in each case some attention was given to the effect of bending freedom of the wing. Thus, although gust problems belonged principally to the aerodynamic-inertia force field, elastic effects were beginning to appear in the theory: in Fig. 2 this is indicated by a tentative (dotted) bond to the elastic force vertex.

5.2.4. *Mechanical Vibration.*

Ten years ago this subject was still in its infancy. In the early days of flying, mechanical vibration does not appear to have presented a serious problem. Probably this was due to the small powers involved (which imply also fairly small amplitudes of impressed force) and to the relatively high damping of the wooden construction then common—particularly since this involved many bracing wires, joints, and so forth.

With the coming of cantilever wings, with the replacement of wood by metal, and with steadily improving methods leading to a more integrated form of construction, the internal damping available had steadily decreased. At the same time, power plants had developed greatly; so that, given a coincidence between an airframe frequency and a powerful engine harmonic, unpleasant vibration was almost bound to develop.

At the time we are considering, Constant's work[15] on thresholds of unpleasantness in vibration was underlining the existence of the problem, while various facets were attracting the attention of Carter and of Morris. But theoretical solutions of the problem were very restricted. They usually involved some far-reaching assumptions—*e.g.*, in engine-air-

screw vibration, that the engine bearers were attached to a rigid body and that the airscrew could be regarded as a rigid disc—which rendered the solution of doubtful general applicability.

At the same time, even these solutions were almost entirely restricted to the determination of natural frequencies of vibration, for the very good reason that there was available very little reliable information on the forcing torque impulses or on the damping; with the result that no serious attempts to forecast amplitudes of response were possible. In view of these considerations it was obviously not expedient to introduce air forces into the problem, and no attempt to do this had been made.

5.2.5. *Impact Problems.*

Up to the time we are considering, landing impacts had been treated on the basis of inertia loads only: that is to say, the aircraft (apart from its undercarriage) was treated as rigid, while aerodynamic forces were ignored in view of the low landing speeds involved. However, it was realised that while these assumptions might be justifiable so long as wings were light and possessed a fairly high flexural frequency, the increasing weight of wings required a reconsideration of the picture. A paper by Fairthorne[16] drew attention to this and made a first examination of the effects of wing flexibility on landing impacts. Aerodynamic forces, however, still played no part in the theory. The bond to inertia forces, and the tentative bond to elastic forces, are indicated in Fig. 2.

6. DEVELOPMENTS IN THE SUBJECTS DURING THE PAST DECADE.

We must here content ourselves with only a brief summary of the main developments which have occurred during the past decade, in so far as such developments are related to the science of aeroelasticity. While war conditions provided the impetus which resulted in the developments concerned, these same conditions have prevented the publication of most of the researches—indeed, many have not been fully reported in any form; and this adds considerable difficulty to the task of a reviewer. However, as we are concerned in the main with general trends, we must regard the subjects touched on as illustrative only, and not as component parts of a detailed picture.

We may naturally enquire at this point what are the physical reasons underlying the developments in our field. In a word, the answer is speed. The remarkable developments in power plants—first in reciprocating engines, then in jet propulsion units—coupled with continued efforts in the direction of drag reduction, have resulted during the past decade in the first real excursion of aircraft into the much discussed but highly speculative realm of compressibility effects.

At the same time, the strength questions posed by this speed increase have been solved, partly by material developments, partly by improved constructional methods, but in the main by a steady movement towards the ideal of the flying wing: *i.e.*, by a steady process of transference of mass from the rest of the aircraft to the wing, in order to obtain the maximum relief from inertia loads. As a result, the elastic properties of aircraft, at one time a secondary or even a last-minute design consideration, have in many cases come to the forefront as one of the principal factors in structural design.

This growing importance of aerodynamic and elastic forces, implied in the design requirements for aeroelasticity,[17] has been strongly underlined, not only by the developments of aeroelastic theory itself, but also by the way in which the implications of elastic deformation (which introduces both aerodynamic and elastic forces) have obtruded themselves on the notice of those concerned with branches of theory in which such deformations could previously be ignored.

In the next paragraphs (6.1 and 6.2) we shall try to show how aeroelastic theory is expanding into other fields, and how the subjects in these fields are expanding into aeroelasticity: we shall find there is some overlapping of our discussion, as there is in the subjects concerned.

6.1. Expansion in the Aeroelastic Field.

In the main, the expansion of aeroelasticity in recent years has been in the nature of the growth of existing subjects rather than of the birth of new subjects. The majority of the subjects can still be comprehended within the fields of flutter, divergence, and loss of control: only one new field of major importance, that of effects on aeroplane stability (which is, of course, closely bound up with control effects) has emerged. In consequence, the developments have been largely in the nature of studies of new methods for dealing with the more complicated problems posed by the rapidly accelerating aircraft of the period.

6.1.1. *Flutter.*

We have already said that, in classical methods of studying flutter, it was necessary to limit the number of degrees of freedom to about three. However, one study had been made in 1936 by Duncan, Collar and Lyon[18] of a problem in which six degrees of freedom had been treated.

It is not easy to say precisely how complexity increases with number of degrees of freedom: probably it is not far wrong to suggest that the complexity is roughly proportional to the factorial of the number of degrees of freedom, if any one method is adhered to throughout. Thus an increase from three to six degrees of freedom necessitated new methods, and these were found in the paper considered by the adoption of inverse methods of solution.

Broadly speaking, the change from the classical method is as follows: instead of laying down the physical parameters and the stability condition, and then solving (by means of Routh's criteria or their equivalents) for the corresponding critical speeds and frequencies, one leaves free certain of the parameters and fixes the critical speed and frequency; the corresponding values of the parameters are then found (a process usually involving only the solution of algebraic equations). In this way a curve of critical speed against the values of the parameters may be found, and the appropriate critical speed corresponding to the correct value of the parameter is thus obtained.

This inverse method of solution has now become standard procedure for problems involving many degrees of freedom, and has indirectly been the means of widening greatly the understanding of the flutter problem. Under the next few headings some of the problems which have been treated in recent years are described shortly. We shall not refer here to flutter of wings not carrying engines, since this is dealt with later (6.1.1.5) in relation to stiffness criteria; nor shall we refer to propeller flutter. The latter subject has been extensively studied experimentally, but no adequate general theories have yet been proposed; and indeed the origin of flutter of metal blades remains in doubt.

6.1.1.1. *Elevator Flutter.*

We shall here restrict our discussion to symmetrical elevator flutter. Purely as a matter of interest, however, it may be noted that aeroelastic science had its beginnings in a study of antisymmetrical elevator flutter, made in 1916—thirty years ago—by Bairstow and Fage.[19] Following the particular occurrence they studied, the port and starboard halves of elevators were given stiff interconnections, and cases of antisymmetrical flutter have since been very rare.

Originally, studies of symmetrical elevator flutter involved two degrees of freedom only: fuselage bending and elevator rotation.

Calculations on this basis had, however, always given numerical results of the wrong order, even allowing for the lack of knowledge of the appropriate aerodynamic forces. In a study of the problem made under the direction of Jahn[20] at the R.A.E., therefore, the following additional degrees of freedom were successively introduced: pitching of the aircraft, vertical translation of the aircraft, and wing bending. (In this particular case two further freedoms, involving elasticities present in the circuit, were also added.) Parallel with this investigation was an experimental research conducted at the N.P.L. by Scruton.[21]

The results obtained in this way were very satisfactory, and design recommendations based on the studies were found to be beneficial. From the motions occurring in the critical condition, it was also found that the problem could be treated rather more simply by considering the motion to be built up from the normal modes of vibration of the aircraft as found in a resonance test.

The implications of this latter finding are of great interest and importance. It will be seen that the degrees of freedom involved are not only the normal modes of vibration—it may be noted here that Duncan[22] had suggested the use of normal modes of vibration as an approach to complex problems—but also the rigid body freedoms (which may in a sense be regarded as normal modes of zero frequency).

Thus the investigation pointed to the fact that an instability having its origin in the tail organs, involving as it does appreciable wing bending and bodily movement, can no longer be treated as a local affair, but must be regarded as "elevator-aeroplane" flutter. Or, in more general terms still, the phenomenon must be regarded as an aspect of the stability of a deformable aeroplane.

6.1.1.2. *Flutter of Wings Carrying Engines.*

This subject has always held considerable interest for flutter investigators. It was realised that the "mass-balancing" effect of engines, as normally disposed, must be considerable; but no quantitative estimates of the effects had been made. Research into this question has been vigorously prosecuted both at the N.P.L.[23] and at the R.A.E.,[24] as well as by certain aircraft firms. The problem is as yet far from final solution; but it is already apparent that a large number of normal modes must be introduced if an adequate solution is to be obtained.

6.1.1.3. *Antisymmetrical Wing Flexure-Torsion Flutter.*

In this phenomenon, as in others, it has been found that it is necessary to introduce not only the antisymmetrical normal vibration modes, but also the relevant rigid body freedoms—in this case freedom in roll.

6.1.1.4. *Mass-Balancing and Flutter Prevention Devices.*

In addition to the studies of elevator flutter referred to in 6.1.1.1, cases of flutter of ailerons, rudders and tabs have also been extensively studied. These studies, while underlining the necessity of treating deformation of the aircraft as a whole as a constituent motion, have also yielded much valuable information on mass-balance as a cure for control surface and tab flutter.

It has been shown, for example, that the virtual inertia of the air[25] must be taken into consideration in the determination of the appropriate inertia characteristics. Another very important result relates to the positioning of mass-balance weights. If it is desired to achieve mass-balance by the use of masses not attached to the control surface, but operated through a linkage, then the position of the mass in relation to the nodes in the vibration modes of the aircraft must be accurately known.[26]

For a balance mass operates as such in virtue of the accelerations imposed on it by the vibration of the aircraft, and if it is

positioned near a node this acceleration will be small; the mass is then practically inoperative as a balance. In fact, if it is on the wrong side of the node it becomes an anti-balance mass. The node may be regarded as a virtual hinge; and if the balance mass is to play its proper part, it must be appropriately positioned in relation to this virtual hinge and the control surface hinge.

In the study of mass balancing of elevators and rudders in particular, it has been necessary to consider as part of the practical problem the effects of the balance masses on longitudinal and directional stability (in the classical sense). Thus there is, in a way, a dual linkage with stability and control matters; not only is the flutter problem becoming a problem in the stability of a deformable aeroplane, but the curative measures adopted must be considered in relation to the stability of the rigid aeroplane.

Considerations similar to those mentioned above apply also to the mass-balance of spring tabs. In this case the problem is complicated by the appearance of elastic couplings, and if the effects of these are to be overcome, the tab must be treated as rotating about its own hinge and a virtual hinge fixed by the geometry of the system. Here again, the balance mass must be correctly positioned in relation to these two hinges; this consideration has led to the definition of a " limiting length of balance arm "[27] and to great clarification of the problem of spring tab flutter.[28]

Studies of methods of prevention of flutter other than mass balance have also been prosecuted, but without very promising results. The introduction of motional forces, or damping, does not seem likely to provide a practical solution for a variety of reasons: the damping required varies acutely with many of the parameters involved, such as height and control surface inertia, and in any case would require prohibitive effort from the pilot if applied directly to the control surface. Irreversible and quasi-irreversible units still have promise,[29] and with careful attention to details of design may in time be adopted as alternatives to mass-balance.

6.1.1.5. *Researches on the Aerodynamic Forces of Flutter.*

While, as indicated above, methods of dealing with the flutter problem have steadily improved and extended, studies of the relevant aerodynamic forces have also been prosecuted. At the N.P.L. a number of investigations have been made into the validity of the constant strip theory derivative coefficients by observations of the flutter characteristics of a series of straight-tapered wings; and some modifications have been proposed.[30] At the same time Jones[31] has made a number of detailed theoretical investigations of the aerodynamic characteristics of oscillating wings of finite aspect ratio and varying planforms.

Some attention has also been given to compressibility effects. In the subsonic field investigations by Frazer[32] and by Jahn have indicated how increasing Mach number may affect critical flutter speeds. In the supersonic field, the aerodynamic forces have been studied by Collar[33] and by Temple and Jahn,[34] and wing flutter calculations for supersonic conditions have been made.

6.1.2. *Divergence.*

There has been no evidence that divergence represents a problem of acute practical importance, and in consequence relatively little attention has been paid to this phenomenon. It has not been entirely forgotten, since there has been a general tendency for wing flexural axes to move aft, and divergence speeds have therefore tended to become lower. In particular, studies have been made of the divergence speed of an elastic wing by iterative methods.[35, 36] The result still emerges, however, that divergence speeds are sufficiently high not to present practical problems.

If divergence does become a problem of

practical importance then, as remarked in 5.1.2, it will become necessary to introduce either a specification of the lift conditions or —since the varying lift will probably be unknown—to treat the response of the aircraft as an integral part of the problem. In this case inertia forces, usually excluded in studies of divergence (since the inertia forces corresponding to the deformation are negligible) will reappear in the problem.

Wing-aileron divergence has also been studied;[37] but here the remarks in 5.1.2 still apply.

Tailplane-elevator divergence speeds have received attention for the first time, in a study of loss of elevator control: this is dealt with later in 6.1.3.2.

6.1.3. *Loss and Reversal of Control.*

It is convenient to divide this topic under two headings: loss of aileron control and loss of elevator control.

6.1.3.1. *Loss of Aileron Control.*

The question of loss of aileron control has had to be kept continuously under review throughout the past decade, since the wing stiffness determined by the associated requirement has often been greater than that required by the wing stiffness criterion (which has assumed more and more the role of a flutter preventive).

Up to a certain stage the work on aileron reversal was ably summarised by Victory.[38] Since that time the increasing importance of compressibility effects, and the possibilities of flight at supersonic speeds, rendered a new investigation desirable; more particularly since wing tapers and stiffness distributions were becoming very different from those of the orthodox wing of ten years ago.

Accordingly, a study of the rolling power of an elastic wing was undertaken by Collar and Broadbent,[39] and the results were applied to flight at subsonic and supersonic speeds:[40] the aerodynamic forces being those given by Glauert[41, 42] in the subsonic range and by Collar[43] in the supersonic range.

The results indicate the very great importance of torsional stiffness for aircraft flying in the neighbourhood of the sonic speed, and show further how inadequate may be the stiffness found by the conventional test method as a measure of the effective stiffness of the elastic wing; since the deformation modes under a concentrated torque and under aerodynamic loading may be widely different.

While, therefore, it is apparent that the subject of aileron reversal and loss of aileron control has progressed considerably, at the same time it has been restricted to unaccelerated conditions; and inertia forces have been excluded from the researches. This has largely been due to the importance assigned to a high steady rate of roll as a criterion of aileron performance.

Nowadays rates of roll are becoming so high that there are signs of a change in this respect; and it is probable that aileron performance will in future be judged by the rapidity with which a vertical bank can be reached, or by some similar criterion. Such a manœuvre will involve acceleration and deceleration in roll; and studies of loss of control due to wing deformation may well have to include these inertia forces in the future.

6.1.3.2. *Loss of Elevator Control.*

With the increasing speeds of ten years ago, and in the absence of requirements for the stiffnesses of fuselage and empennage, it was to be expected that aeroelastic effects on the tail organs would make themselves felt. The first evidence of this was through effects on longitudinal stability, and though no cases of actual reversal of elevator control have been reported, observed tail deformations and a crop of associated troubles rendered an investigation of those effects necessary.

The investigation, made by Collar and Grinsted,[44] treated tailplane twist as the main variable, but examined also the effects of elevator twist and of fuselage flexibility under

shear and moment loads. It was shown that severe structural distortion might result unless the elevator reversal speed were of the order of twice the maximum speed, and that in consequence high standards of stiffness for the fuselage and empennage were very desirable. The tailplane-elevator divergence speed was also studied—a phenomenon of a somewhat different character from wing-aileron divergence.

Finally, the investigation included a treatment of the effect of tailplane twist on longitudinal static stability, which showed how if the tailplane stiffness was inadequate a stable aeroplane might at high speeds become more stable or might develop instability, depending on tail setting, wing pitching moment and so forth.

This investigation was followed by a second study in the same field by Collar and Victory.[45] The object here was to define, in the form of stiffness criteria, desirable standards of structural stiffness for tailplanes, elevators and fuselages, and to examine how far these stiffnesses were interdependent in their effects on elevator control.

The question was approached theoretically by a consideration of loss of control and more particularly with a view to limitation within reasonable bounds of the structural distortions due to application of elevator. By this means appropriate forms for the criteria were derived, and the required order of magnitude indicated.

To supplement this a statistical survey* was also made of the experimental evidence on all these stiffnesses—evidence accumulated in a somewhat desultory way over a number of years: and appropriate criteria were defined and proposed for adoption.

6.1.4. *Elastic Effects on Stability.*

It has been indicated above that this problem has appeared for the first time during the last decade. It might have been thought that the principal practical problem would have been one of lateral stability, resulting from the changes in effective dihedral produced by wing bending; but in fact the effects of elastic distortion on longitudinal stability have proved much more serious.

The previous problem had been envisaged by Pugsley, and Bryant and Pugsley[46] demonstrated how, with increasing wing loadings, the values of wing dihedral would have to be carefully watched if lateral instability were to be avoided. But further expansion of aeroelastic theory in this field does not appear to have been required by experience.

Regarding longitudinal stability, an early examination of the effects of wing torsion was made by Pugsley.[47]

Reference has already been made to the extension of the aeroelastic field to include loss of elevator control and to the study by Collar and Grinsted of the effects of tail and fuselage distortion on longitudinal stability. Further studies in this area are largely due to Gates, Lyon, and their collaborators, and are more in the nature of introduction of elastic effects into general stability studies; and, as such, will be dealt with later.

6.1.5. *Developments in Stiffness Criteria.*

We have already remarked that, when Pugsley reviewed the subject of aeroelasticity, there was virtually only one stiffness criterion: and even that criterion had not been accepted for current use except as recommended practice. This criterion laid down, in very simple terms, the wing symmetrical torsional stiffness, measured between the wing root and the mid-aileron section.

* The word "statistical" is used somewhat loosely here and elsewhere. In nearly all such surveys of stiffness criteria the data are so varied in nature and origin, and so scanty, that it is not possible to obtain frequency distributions in the strict statistical sense: the best that can be done is to weigh the data on a basis of experience, common sense, and a knowledge of the origins of each item, and to take an arithmetic mean. To be honest, it is often difficult to avoid an element of casuistry in the process, since the data are often being used to test a theoretical result.

During the period under review, some changes have developed in this criterion and new proposals have been made. At the same time, new criteria have appeared; it is convenient to discuss these aspects under separate headings.

6.1.5.1. *The Wing Stiffness Criteria.*

The developments in the mid-aileron torsional stiffness criterion have recently been reviewed,[48] and new proposals made; we shall here make only a brief précis of this work.

First let us note that for conventional wings it has not been necessary to lay down any standard for bending stiffness: this in spite of the fact that, partly as a result, the ratio of bending to torsional stiffness has decreased on the average to about one-fifth or less of the value it had ten years ago. It would seem probable that the design of the wing for strength leads almost automatically to adequate bending stiffness. This view is supported by a survey of bending stiffnesses, which shows smaller variations in a bending stiffness criterion, from one aeroplane to another, than might have been expected if there were no controlling factor.

On the other hand, two criteria for wing torsional stiffness have been current for some years, having been made mandatory shortly before the war: these are the " mid-aileron " and the " equivalent tip " criteria. Only the first of these has been important in practice: satisfaction of the mid-aileron criterion has almost invariably implied satisfaction of the equivalent tip criterion by a wide margin.

Regarding the mid-aileron criterion, we may revert to Pugsley's speculation, referred to in 2, as to whether this would be defined by reversal of aileron control, wing flexure-torsion flutter, or wing divergence. The answer, so far as the past ten years are concerned, is wing flexure-torsion flutter.

Reversal of aileron control has had its own criterion, in the sense that it is a requirement that the aileron reversal speed shall exceed the maximum diving speed by an adequate margin; while it has not been possible to raise the flexure-torsion flutter speed economically by any means except that of increasing the wing torsional stiffness. It follows that the divergence speed has in practice remained at a higher level than the flexure-torsion flutter speed, and the latter has therefore defined the criterion.

Thus the mid-aileron wing torsional stiffness criterion has assumed almost exclusively the role of a preventive of flexure-torsion flutter. This criterion is remarkably simple in form, since it involves as parameters only the maximum equivalent airspeed, the mean chord and span, and the torsional stiffness. No account is taken of parameters which are in fact known to affect the flutter speed appreciably, such as wing taper, bending stiffness or position of the flexural axis. Even the most important parameter—inertia distribution—is left out of account except in that different values of the criterion are laid down for wings with and without wing engines. Finally, compressibility effects are ignored.

The great simplicity of the criterion, and its uniform success in preventing trouble, rendered any modification undesirable so long as no great weight penalty was incurred in its satisfaction (it must be remembered that a certain standard of stiffness, of the same order, is in any case required by aileron reversal considerations). This fortunate state of affairs has continued until quite recently.

In the past two or three years, however, an increasing tendency towards strong wing taper has produced wings in which the natural tendency is for the local wing torsional stiffness to fall off very rapidly towards the tip; and this and other factors demanded that the form of the criterion be reconsidered. To this end a number of investigations into the effects of the parameters concerned have been made.

It has been demonstrated that the optimum reference section for the measurement of the

torsional stiffness is a function both of the modes of distortion assumed and of the taper, but that a section at about 0.7 span can safely be used. Investigations have also been made of the variation of critical flutter speed with taper, with assumed modes of twist and of bending, with wing density and inertia axis position, and with wing bending stiffness and flexural axis position. Account has also been taken of the difference between dynamic and static stiffness.

All these investigations have recently been reviewed,[48] and the results have been extended and incorporated in a proposed new criterion. This new proposal is an elaboration of the existing criterion, additional terms introducing the more important parameters such as taper, inertia axis position and Mach number.

At present, a survey of values of the proposed new criterion achieved by existing aircraft is in progress.

6·1.5.2. *Additional Criteria.*

Requirements now exist,[17] in the form of criteria, for torsional stiffness of ailerons, elevators and rudders. These criteria are all similar in form: this form being that derived from the theoretical studies of loss of elevator control mentioned earlier.[44, 45] The numerical values to be achieved are, however, different, being based on practical results rather than on theoretical predictions.

In the case of ailerons two values are defined, one for ailerons with distributed mass balance and the second for other ailerons; similarly, different values are required for elevators with and without horn balances.

In addition there is a requirement for the flexural stiffness of the overhang of an elevator beyond its outermost hinges.

With the exception of wings probably the most important stiffnesses are those of the tailplane in twist and of the fuselage in vertical flexure: and criteria have been defined for these stiffnesses. Although the wing and tailplane torsional stiffness criteria are identical in form, the tailplane criterion is not determined by flexure-torsion flutter considerations, while the wing criterion is. The tailplane criterion is fixed largely by considerations of longitudinal stability and control; it is noteworthy that the two criteria have, however, nearly the same numerical values. On the whole, the tailplane criterion is slightly more severe than the wing criterion, and it is probably this fact, together with the simpler structural problem, which has resulted in the practical freedom of tailplanes from flexure-torsion flutter.

Two tailplane criteria are defined, one for tailplanes without outboard fins and rudders and a rather more severe criterion for tailplanes carrying outboard fins and rudders.

The fuselage stiffness in vertical flexure is also determined from considerations of longitudinal stability and control.

Corresponding to vertical fuselage flexure and tailplane twist are lateral fuselage flexure and fin twist. No theoretical investigations into desirable values for the stiffnesses in this second case have been made; but a lateral stiffness criterion for fuselages has been defined, by analogy with the vertical stiffness criterion, and a value assigned to it on the basis of practical measurements.

It has not been found practicable to define a criterion for fins, in view of their very wide variations in shape and attachment—particularly in the case of outboard fins.

The last major structural criterion is that for torsional stiffness of fuselages: it is this criterion which would enter into calculations of antisymmetrical elevator flutter.

Finally, standards for the stiffnesses of control circuits have been laid down. These take the form of a requirement that the stretch produced by a reasonably big effort on the part of the pilot shall not exceed more than a given percentage of the total available movement. There is in addition a stiffness requirement for the aileron balance circuit,

designed to ensure that serious upfloat of the ailerons shall not occur.

6.2. Expansion in Fields Allied to Aeroelasticity.

In 6.1 we indicated how aeroelasticity has been, in effect, expanding into other fields. In the present paragraph we shall attempt to show something of the reverse process.

6.2.1. *Elasticity in Problems of Stability and Control.*

We have already shown how elastic effects enter into both lateral and longitudinal control. In lateral stability, however, elastic effects do not appear to have presented a serious problem: such effects undoubtedly exist, since, for example, under normal acceleration the wing dihedral must change very appreciably. These problems of lateral stability have been kept in mind; but it is in the field of longitudinal stability that the effects of elastic deformation are most pronounced and can have serious consequences.

The treatment of the problem of longitudinal stability has been greatly simplified by the expositions by Gates of the fundamentals of longitudinal stability[49] and of the idea of manœuvrability.[50] By studying independently the static stability, stick fixed and free, and the manœuvrability, stick fixed and free, the important aspects of longitudinal stability of an aircraft are all covered. Without this subdivision, the introduction of elastic effects into longitudinal stability investigations would have involved much more arduous calculations and made the interpretation of the results much more difficult.

As it is, Gates and Lyon[51] and their collaborators have now proposed a theory in which the effects on all aspects of longitudinal stability of elastic deformations in the fuselage and empennage are taken into account;[52] and following the earlier precedent[44] have absorbed into stability theory the appropriate stiffness criteria and the elevator reversal speed.

It is in the field of longitudinal stability that another important deformation effect has first become serious: that of panel distortion under load.[53] In elevators, the distortion of the panels of the structure under aerodynamic loads can produce sufficient change in the effective camber to result in quite abnormal hinge moments; and the stick free stability can be adversely affected to a serious extent. To a lesser extent, panel deformation in ailerons can also be serious.

It may be remarked that the deflection of a panel is obviously not proportional to the applied load; and thus panel deflections have for the first time introduced serious elastic non-linearities into aeroelastic problems, and we can no longer talk with accuracy in terms of stiffnesses or stiffness criteria. How this difficulty is to be met is a matter still under consideration.

6.2.2. *Elastic Effects in Loading Problems.*

In 5.2.2. we stated that up to ten years ago loading problems were treated on the aerodynamic-inertia force basis, and that elastic deformations were in the main ignored. In this area, the position has not changed radically, and we are not yet being driven to a correlation between strength and stiffness. Nevertheless, there are indications that we are moving towards such a correlation, and we shall try here to indicate the roads by which this movement is taking place.

In the first place, both aeroelastic requirements and aerodynamic requirements for smoothness of profile are demanding much greater skin thicknesses than were common ten years ago. In consequence, the skin is rapidly becoming a main stress-bearing part of the structure, and it is not impossible that we shall soon see spars disappearing from wings in favour of all-skin designs. In this event the strength and stiffness will be much more closely tied together than is at present the case.

Again, there is an increasing realisation that the structural strength of an aircraft can no longer be adequately treated on the basis of static loads alone. Much attention has been given, and more is required, to the problem of fatigue of built-up structures; and the oscillatory loads involved may be of aerodynamic origin as well as due to mechanical vibration.

In this connection it may be remarked that proposals are in hand for strength testing an airframe under conditions designed to simulate those which would obtain during an occurrence of flutter. In this field of non-static loads we may also note the work which has been done on the strength of structures under repeated slow loading; although this has not resulted directly from elastic effects, it has an obvious bearing on strength under oscillatory loads of aero-elastic origin.

6.2.3. *Gusts.*

Knowledge of the structure of gusts has not increased to any appreciable extent during the period under review, and in consequence calculations still have to be made for somewhat arbitrary gust cases. But the methods of dealing with these cases have considerably developed.

This is best illustrated by reference to a calculation made by the Bristol Aeroplane Company in relation to one of their aeroplanes; in this calculation, the growth of the air forces according to Wagner's theory, the response of the aeroplane, and a large number of elastic freedoms for the structure, were all taken into account in some detail—a complete aerodynamic-elastic-inertia force problem.

At the same time, since gust cases are often severe, attention is being devoted by many investigators to the problem of cheating the gust. One way of doing this is to employ the elastic deformation of the wing to reduce the adverse loadings. For example, if it were possible by a structural artifice to obtain an effective flexural centre near to or ahead of the leading edge, then a gust load would cause a change in wing incidence near the tips in the sense required to reduce the applied load there; and other examples might be given.

Here, then, is a problem in which design for strength may be appreciably affected by the design for stiffness; and, it must be added, such a design would have to be watched carefully from other aeroelastic viewpoints, particularly those of flutter and of stability.

6.2.4. *Mechanical Vibration.*

In this field considerable advances, which owe much to the energetic researches directed by Morris,[54] have been made in the past ten years. We cannot enlarge on these advances here, since up to the present time the researches have been confined to the inertia-elastic field. We may, however, note that the stage has now been reached when the determination of the natural frequencies of a complete aircraft, and particularly those frequencies which seem likely to be important from the vibration viewpoint, can be predicted with fair confidence.

Thus the investigators have reached a milestone in the road of their research; and the next stage must be the determination of amplitudes of response by the inclusion in the equations of motion of the forcing impulses arising in the engine and of the air forces; since the latter provide both damping forces and modifications to the elastic stiffnesses.

That mechanical vibration and flutter are beginning to overlap is illustrated by the following occurrence. On a particular aircraft the elevator trim tabs were not mass-balanced, since the backlash in the operating system was very small. On one mark of the aircraft, which had one particular type of engine, no trouble occurred. But on a second mark, with a different engine, very unpleasant vibration developed, and was cured by reducing the backlash in the tab to a much smaller amount than had previously

been thought necessary. It was evident, therefore, that the different forcing impulses from the second engine were producing a tab flutter, within the limits of the tab backlash, where previously none had existed.

6.2.5. *Impact Effects.*

As in the case of mechanical vibration, the researches on this subject have been confined, up to the present time, to the inertia-elastic field;[55, 56] and some important effects, particularly on the increase in local stress above that given by the rigid aeroplane case, have been observed.

Although increasing attention is being paid to this subject it has still not yet reached the stage where even on the restricted inertia-elastic force basis, accurate quantitative predictions of the important local stress variations resulting from a landing impact can be made. When this stage is reached it will be expedient to introduce the air forces also; for, as speeds at touch down steadily increase, these forces are becoming correspondingly more important, and may provide a significant correction to the stress calculations.

7. THE FUTURE.

We have seen in 6 how the individual studies with which we are concerned are expanding; and we may revert to our "triangle of forces" diagram to examine what is happening to the subjects there described. We may perhaps take the subject of aeroplane stability as an example.

In Fig. 2 this subject is bonded to aerodynamic and inertia forces only, and is unconnected to the third vertex of the triangle—elastic forces. In consequence, it lies outside the aeroelastic triangle. But, at the present day, it has acquired a bond to the third vertex—not perhaps so strong as its connection with aerodynamic and inertia forces, but nevertheless sufficient to draw the subject within the general domain of aeroelasticity. We may, in fact, re-draw our triangle as shown in Fig. 3.

In this triangle we see how most of the subjects have now become bonded—although at present the bonds are often tentative—to a third vertex, where previously only two such bonds existed; moreover, certain of the subjects are becoming bonded together by obvious interconnections. Thus, for example, loss and reversal of elevator control is obviously directly connected with longitudinal stability; while the study of gusts has much in common with both flutter studies and investigations of landing impacts, and so on.

In short, it is evident that we are no longer dealing with a series of subjects, each in its own watertight compartment: there is a definite coalescence of the subjects into an integrated whole, which may be defined as the dynamics of a deformable aeroplane. And we are faced with the question: what are to be our methods of treatment of this unified problem? It is obviously not possible to be dogmatic on this important question; but the writer would suggest the following possible answers.

7.1. Treatment of the problem.

To the mathematician the description of the subject—dynamics of a deformable aeroplane—suggests the formal solution at once. We select as generalised co-ordinates quantities defining the translational and angular freedoms of the aeroplane as a rigid body; we add co-ordinates representing the control surface angles, tab angles, stick and pedal movements, and with these we include co-ordinates to represent the freedoms due to automatic controls; and finally we introduce a number of co-ordinates sufficient to describe the modes—normal or otherwise—of elastic deformation of the aircraft. We then derive, in terms of the inertia and elastic properties of the aircraft and the air forces, the complete Lagrangian equations of motion, including in the appropriate equations forcing terms representing mechanical

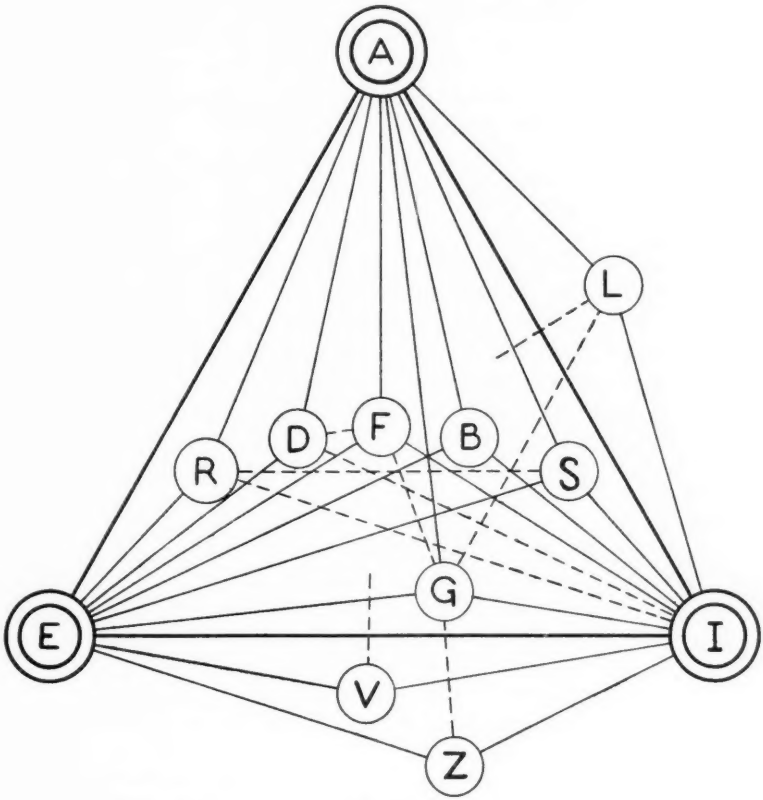


Fig. 3

A : aerodynamic forces
E : elastic forces
I : inertia forces

F : flutter
B : buffeting
S : stability and control
D : divergence
R : reversal of control
G : gusts
L : loading problems
V : mechanical vibration
Z : impacts

vibration impulses, gusts, landing impacts, pilot's efforts, and so forth.

In this way, we obtain a set of simultaneous differential equations, probably in twenty to thirty degrees of freedom, which describe completely the motion of the aircraft and which can tell us whether serious trouble is likely to arise or not.

From the practical viewpoint, of course, the labour of dealing with such a set of equations is prohibitive at present. But we must not dismiss out of hand the possibility of solving such a set of equations, at least so long as they are linear in the unknown co-ordinates or approximations to linearity can be safely made. For we have already remarked that inverse methods of solution have remarkable power; and by adoption of such methods of dealing with the equations we may well be able to reduce the problem to that of evaluation of numbers of numerical determinants of large order.

At present this process would be far too arduous and would offer such great possibilities—even probabilities—of error that no attempt would be worth while. But in the last few years considerable strides have been made in dealing with large numbers of simultaneous equations by the use of electrical, mechanical, and electro-mechanical machines;[57, 58] while the growing use of matrix algebra[59] makes the formulation of the problem, as well as its solution, much easier.

It is thus not outside the bounds of possibility that within a period of years we may have available computational aids of such power that the solution of equations of the kind we are considering may be a routine process.

In the writer's view it is more probable that the chief difficulty will be, not in the solution of the differential equations, but in the derivation of the equations themselves from the basic data: even at the present time, in advanced flutter calculations, the solution of the equations is the simpler half of the work. However, the formulation of the equations may not be so much more complex than at present, since in the majority of cases the number of coupling terms will probably be relatively small.

Meanwhile, we shall obviously continue to discuss our subjects as separate entities. The fact that the coupling terms in the equations of motion are few (or weak) has permitted us to regard the subjects as separate in past years, and there is no reason why we should not continue to take advantage of this fact (provided that we are careful to assess the magnitude of those couplings we ignore) for some time to come. But it may well be that a reconsideration of programmes and methods at this stage is worth while.

We may, for example, take the subjects we have discussed and, since they are becoming unified, try to relate them according to some particular characteristic: one such characteristic that suggests itself is frequency of oscillation. On this basis we could prepare a table with a spectrum of frequencies such as follows:—

Subject	Frequency
Reversal of control Divergence	Zero
Loading problems Stability	Zero or low
Gusts	Low or medium
Impacts Flutter	Medium or high
Vibration	High

On inspection of this table we see that it may be possible to solve our unified problem by unified treatment, but that it is probably not necessary to take the whole group at once. Thus the connection between the first and last subjects is negligible: we need not consider vibration questions in dealing with reversal of control, and *vice versa*. But in dealing with reversal of control we have a strong connection with stability studies: in stability studies we have a growing connection with flutter; and flutter and mechanical vibration are becoming closer: so that the tie between first and last exists.

Thus in the future, given our computational aids, it may well be that we shall write down for each subject a sufficient number of equations of motion to cover appropriate neighbouring subjects in the spectrum. In studying reversal of control we may include divergence and stability: in studying stability we may include reversal of control and flutter: in studying flutter we may include stability and mechanical vibration.

Viewed from this angle, it appears that the unification of the subject does not necessarily involve a hopeless complexity but offers rather the advantage that we may study the various aspects in relation to their associated subjects by a standard method of attack.

7.2. Standards of elasticity.

We have speculated in the last paragraph on how our subject may be treated in the

future; we may now perhaps be permitted to speculate further on the methods of curing trouble as it arises. And since the aerodynamic and inertia loads are likely still to be dictated mainly by other considerations, the principal control on aeroelastic and associated phenomena will continue to be exercised through stiffness provisions. We may make more use of inertia distributions than in the past, but except for mass-balance of control surfaces to prevent flutter, variation in inertia distribution is likely to be a supplementary measure to the main process of stiffness variation.

In the first place, it would appear that the simple stiffness criteria which have served us so well in the past are likely to require elaboration. We have remarked that this elaboration has already been proposed for the wing torsional stiffness criterion; although it does not at present amount to more than saying that the familiar criterion is no longer a constant, but is a function of all the important relevant parameters.

On a point of detail, it may be found to be desirable to square the criteria in their present form and to introduce the standard air density to render the criteria non-dimensional; if this were done the criteria would be in the nature of force or moment coefficients corresponding to unit increments of displacement, and would define the required stiffnesses explicitly.

But we may lose even this elaborated form as we modify our designs to penetrate further and further into the sonic regions and beyond. For not only must we introduce different functions for Mach number corrections at subsonic and supersonic speeds; we are also likely to find that new criteria are involved and that there is such an essential interdependence between the criteria that we cannot define them separately.

We are beginning to realise this in studies of sweptback wings for high speeds:[60] wing bending stiffness becomes increasingly more important, as sweepback increases, in such matters as aileron reversal, longitudinal stability and flutter. And the bending and torsional stiffnesses are almost indissolubly mixed in the theories—it is in fact no easy matter even to formulate unambiguous definitions of bending and torsional stiffness for cranked wings.

If, therefore, we find our simple stiffness criteria unsuitable or insufficient for design purposes we may have recourse to a process of " stiffness stressing "—a study by stress offices of deformations under various loading conditions, parallel to the studies of strength now made. In this way we may find a new correlation between strength and stiffness coming about. But in any case, it seems evident that for very high speed aircraft the utmost efforts should be made to ensure that elastic deformations are minimised; and, *ceteris paribus*, those aircraft having high structural stiffness are likely to be most successful.

8. CONCLUSIONS.

In what has gone before we have attempted to review in a very small compass the progress in the past decade of aeroelasticity and its allied subjects. The writer is aware that, in attempting this task, he has laid himself open to a charge of insularity; and, indeed, some may regard the reference list as parochial.

As regards the latter charge we may, perhaps, be allowed to remark that during the past ten years the number of papers presented to the Oscillation Sub-Committee of the A.R.C. alone is nearly six hundred; and almost without exception these papers have been related to aeroelasticity in the wider sense implied in the present paper.

The number of relevant communications to other bodies is not known. But it is apparent that in any attempt to survey the general trend of work in all areas, it is necessary to make a very restricted choice of references; in the present case it is hoped that those quoted will themselves give the

interested reader more extended references in their own areas.

In the flutter field, a bibliography of British work containing about two hundred and fifty references was recently prepared by Graham;[61] other similar bibliographies in all fields would be very helpful.

The charge of insularity is well sustained and cannot be refuted. In explanation, however, we must point out that, except in the flutter field, countries other than Great Britain have given little attention to aeroelastic matters until the past few years. Only recently has it become the practice to examine on a more or less routine basis the effects of distortion on lateral and longitudinal control and stability.

Indeed, we have recently learned[62] that German military aircraft suffered heavily at one period during the war as a result of the lack of attention paid to aeroelastic phenomena; and much concentrated effort had to be applied to the task of finding out what was going wrong and into the redesign which was found to be necessary in many cases.

Moreover, nowhere else has theory or experiment thrown up a method of dealing with aeroelastic phenomena of a simplicity comparable with that of the stiffness criteria used in Great Britain.

Thus it may fairly be claimed that in co-ordinated aeroelastic research, the most consistent developments have taken place in Great Britain. This is not to say, of course, that in individual areas we have not learned much from other countries: the reverse is the case. To take only one example, in the flutter field the present emphasis on normal modes of vibration as found from resonance tests owes much to American influence. But co-ordinated aeroelastic research belongs peculiarly to Great Britain; and, as the writer has attempted to show, a larger co-ordination may very soon be necessary.

The writer wishes to express his thanks to the Ministry of Supply and Aircraft Production for permission to refer to many Reports which are as yet unpublished and to work for the Ministry on which he was personally engaged during the war years.

References.

NOTE.—It must be clearly realised that the list of references given here is by no means complete; indeed, it is no more than a small sample of the papers which might be consulted. However, in most cases the works quoted themselves contain reference lists covering their own areas reasonably fully.

1. A. G. Pugsley—Control Surface and Wing Stability Problems. J.R.Ae.S. Nov., 1937.
2. R. A. Frazer, W. J. Duncan—The Flutter of aeroplane wings. R. & M. 1155. Aug., 1928.
3. R. A. Frazer, W. J. Duncan—The Flutter of Monoplanes, Biplanes and Tail Units. R. & M. 1255. Jan., 1931.
4. W. J. Duncan, A. R. Collar—Resistance Derivatives of Flutter Theory, Pt. I. R. & M. 1500. Oct., 1932.
5. W. J. Duncan, A. R. Collar—The Present Position of the Investigation of Airscrew Flutter. R. & M. 1518. Dec., 1932.
6. H. Roxbee Cox, A. G. Pugsley—Theory of Loss of Lateral Control Due to Wing Twisting. R. & M. 1506. Oct., 1932.
7. A. G. Pugsley, H. Roxbee Cox—The Aileron Power of a Monoplane. R. & M. 1640. Apl., 1934.
8. A. G. Pugsley—The Aerodynamic Characteristics of a Semi-Rigid Wing Relevant to the Problem of Loss of Lateral Control Due to Wing Twisting. R. & M. 1490. June, 1932.
9. A. G. Pugsley, G. R. Brooke—The Calculation by Successive Approximation of the Critical Reversal Speed. R. & M. 1508. Oct., 1932.
10. Staff of Aerodynamics Dept., N.P.L.—Two Reports on Tail Buffeting. R. & M. 1457. Oct., 1932.
11. S. B. Gates, H. B. Irving—An Analysis of Aileron Performance. R.A.E. Rept. Aero. 1624; A.R.C. 4652. July 1940.
12. H. Roxbee Cox, A. G. Pugsley—Stability of Static Equilibrium of Elastic and Aerodynamic Actions on a Wing. R. & M. 1509. Oct., 1932.
13. L. W. Bryant, I. M. W. Jones—Stressing of Aeroplane Wings Due to Symmetrical Gusts. R. & M. 1690. Feb., 1936.
14. D. Williams, J. Hanson—Gust Loads on Tails and Wings. R. & M. 1823. May, 1937.
15. H. Constant—Aircraft Vibration. R. & M. 1637. Oct., 1934.
16. R. A. Fairthorne—The Effects of Landing Shock on Wing and Undercarriage Deflexion. R. & M. 1877. Jan., 1939.
17. A. P. 970, Part V.
18. W. J. Duncan, A. R. Collar, H. M. Lyon—Oscillations of Elastic Blades and Wings in an Airstream. R. & M. 1716. Jan., 1936.

19. L. Bairstow, A. Fage—Torsional Vibrations on the Tail of an Aeroplane, Pt. II; Oscillations of the Tailplane and Body of an Aeroplane in Flight. R. & M. 276. July, 1916.

20. H. A. Jahn, G. H. L. Buxton, I. T. Minhinnick — Fuselage Vertical Bending — Elevator Flutter on the Typhoon. R.A.E. Rept. S.M.E. 3286; A.R.C. 7744. Mar., 1944.

21. C. Scruton, G. Sim, W. T. Burke—Experiments on Tail Flutter: Effect of Fuselage Pitching Mobility on Symmetrical Elevator Flutter. A.R.C. 6717. May, 1943.

22. W. J. Duncan—A Suggested Investigation on Wing Flutter. A.R.C. 4281. Nov., 1939.

23. N. C. Lambourne, D. Weston—An Experimental Investigation of the Effect of Localised Masses on the Flutter and Resonances of a Model Wing. A.R.C. 7604. Apl., 1944.

24. I. T. Minhinnick, J. Yarwood—Interim Note on the Theoretical Effects of an Engine Mass on Wing Flutter. R.A.E. Tech. Note S.M.E. 143; A.R.C. 6720. May, 1943.

25. A. R. Collar—Note on Virtual Inertia and Control Surface Flutter. R.A.E. Tech. Note S.M.E. 218; A.R.C. 7469. Feb., 1944.

26. A. R. Collar—Mass Balancing of Controls and Flutter Prevention Devices. A.R.C. 8091. July, 1944.

27. R. A. Frazer, W. P. Jones—Wing-Aileron-Tab Flutter, Pts. I and II. A.R.C. 5668. Mar., 1942.

28. A. R. Collar—The Prevention of Flutter of Spring Tabs. R.A.E. Rept. S.M.E. 3249; A.R.C. 6837. May, 1943.

29. A. R. Collar—A Proposal for a Control System Operated by an Aerodynamic Servo and Avoiding Large Mass-Balance Weights. R.A.E. Tech. Note S.M.E. 293; A.R.C. 8411. Dec., 1944.

30. J. Williams—Note on the Fundamental Coefficients for Flexure-Torsion Flutter. A.R.C. 8193. Nov., 1944.

31. W. Prichard Jones—Aerodynamic Forces on Wings in Non-Uniform Motion. A.R.C. 8928. Aug., 1945.

32. R. A. Frazer—Notes on the Influence of Mach Number on Flutter and Divergence of a Tapered Wing. A.R.C. 4949. Feb., 1941.

33. A. R. Collar—Resistance Derivatives of Flutter Theory, Part II. Results for Supersonic Speeds. R.A.E. Rept. S.M.E. 3278; A.R.C. 7470. Dec., 1943.

34. G. Temple, H. A. Jahn—Flutter at Supersonic Speeds, Pt. I: Mid-Chord Derivative Coefficients for a Thin Aerofoil at Zero Incidence. R.A.E. Rept. S.M.E. 3314; A.R.C. 8583. June, 1945.

35. A. G. Pugsley, G. A. Naylor—The Divergence Speed of an Elastic Wing. R. & M. 1815. Oct., 1937.

36. E. G. Broadbent—The Estimation of Wing Divergence Speeds. R.A.E. Rept. S.M.E. 3355; A.R.C. 9493. Dec., 1945.

37. A. R. Collar, M. Victory—Reversal of Aileron Control, Wing Divergence, and Wing-Aileron Divergence for the Case of an Elastic Wing: A Study of Recent American Methods of Calculation. R.A.E. Rept. S.M.E. 3262; A.R.C. 7107. Aug., 1943.

38. M. Victory—The Calculation of Aileron Reversal Speed. R.A.E. Rept. S.M.E. 3279; A.R.C. 7486. Jan., 1944.

39. A. R. Collar, E. G. Broadbent—The Rolling Power of an Elastic Wing, Pt. I: Compressibility Effects Absent. R.A.E. Rept. S.M.E. 3322; A.R.C. 8786. May, 1945.

40. E. G. Broadbent, A. R. Collar—The Rolling Power of an Elastic Wing, Pt. II: Compressibility Effects and Results for Supersonic Speeds. R.A.E. Rept. S.M.E. 3347; A.R.C. 9239. Oct., 1945.

41. H. Glauert—Theoretical Relationships for an Aerofoil with Hinged Flap. R. & M. 1095. Apl., 1927.

42. H. Glauert—The Effect of Compressibility on the Lift of an Aerofoil. R. & M. 1135. Sept., 1927.

43. A. R. Collar—Theoretical Forces and Moments on a Thin Aerofoil with Hinged Flap at Supersonic Speeds. R. & M. 2004. Nov., 1943.

44. A. R. Collar, F. Grinsted—The Effects of Structural Flexibility of Tailplane, Elevator and Fuselage on Longitudinal Control and Stability. R. & M. 2010. Sept., 1942.

45. A. R. Collar, M. Victory—Standards of Structural Stiffness for Aeroplane Tailplanes, Elevators and Fuselages. R.A.E. Rept. S.M.E. 3251; A.R.C. 6818. June, 1943.

46. L. W. Bryant, A. G. Pugsley—The Lateral Stability of Highly Loaded Aeroplanes. R. & M. 1840. Sept., 1936.

47. A. G. Pugsley—Influence of Wing Elasticity upon Longitudinal Stability. R & M. 1548. Jan., 1933.

48. A. R. Collar, E. G. Broadbent, E. B. Puttick—An Elaboration of the Criterion for Wing Torsional Stiffness. R.A.E. Rept. S.M.E. 3357; A.R.C. 9540. Jan., 1946.

49. S. B. Gates—Longitudinal Stability and Control. A Simple Exposition of the Fundamentals Employed in this Aspect of Design. "Aircraft Engineering," Sept., 1940. (Page 260.)

50. S. B. Gates—A Proposal for an Elevator Manœuvrability Criterion. R.A.E. Rept. Aero. 1740. Mar., 1942.

51. S. B. Gates, H. M. Lyon—A Continuation of Longitudinal Stability and Control Analysis. Pt. I: General Theory. R.A.E. Rept. Aero. 1912; A.R.C. 7687. Dec., 1944.

52. H. M. Lyon, J. Ripley—A General Survey of the Effects of Flexibility of the Fuselage Tail Unit, and Control Systems on Longitudinal Stability and Control. R.A.E. Rept. Aero. 2065; A.R.C. 9015. July, 1945.

53. M. B. Morgan, H. H. B. M. Thomas—Control Surface Design in Theory and Practice. J.R.Ae.S. Aug., 1945.

54. J. Morris—The Escalator Method in Engineering Vibration Problems. Chapman & Hall, 1946.

55. D. Williams—Dynamic Loads in Aeroplanes under given Impulsive Loads. A Simplified Method of Calculation. R.A.E. Rept. S.M.E. 3309. Mar., 1945.
56. A. Robinson—Shock Transmission in Beams. R.A.E. Rept. S.M.E. 3319. Apl., 1945.
57. R. R. M. Mallock—An Electrical Calculating Machine. Proc. Roy. Soc. A.140, p. 457. Mar., 1933.
58. R. J. Schmidt, R. A. Fairthorne—Some Comments on the Use of Hollerith Machines for Scientific Calculations. R.A.E. Rept. A.D. 3153; A.R.C. 4721. June, 1940.
59. R. A. Frazer, W. J. Duncan, A. R. Collar—Elementary Matrices. Camb. University Press, 1938.
60. A. R. Collar—Some Problems in the Design of Sweptback Wings. A.R.C. 9264. Jan., 1946.
61. M. E. K. Graham—A Bibliography of British Work on Flutter. R.A.E. Tech. Note S.M.E. 224; A.R.C. 7685. Apl., 1944.
62. A. R. Collar, H. A. Jahn, A. Robinson, E. G. Broadbent—German Aircraft Industry—Aeroelasticity. B.I.O.S. Rept. No. 12, Item No. 25. 1946.

REVIEWS

Basic Mathematics for Radio Students.

E. M. Colebrook, B.Sc., D.I.C., A.C.G.I. Iliffe & Sons, Ltd., 1946. 10/6 net.

" Mathematics," said Bertrand Russell, in his " The Study of Mathematics," " takes us into the region of absolute necessity, to which not only the actual world, but every possible world, must conform."

More than ever during the past six or seven years has it become evident that a sound knowledge of basic mathematics is one of the major keys to open the doors to clear thinking. It is a pity that these doors in the past have been fitted with Chubb locks and the opening of them made as difficult as possible. The teaching of mathematics, particularly in its early stages, has, indeed, been one of the major causes to make the subject one of profound distaste to so many.

All the more welcome, therefore, is any book which breaks away from the mathematical miasmas of the past. Such a book is the present one, and the author is to be congratulated upon it. Mr. Colebrook begins with a chapter on Elementary Algebra; Fundamental Ideas, and ends with the Differential and Integral Calculus, through Indices and Logarithms, Complex Numbers, Continuity, Limits and Series and Geometry and Trigonometry. There is an additional chapter on the Application of Mathematical ideas to Radio. Throughout the book are numerous examples.

Although written primarily for radio students this is a book which is worth while reading by all who have found that their elementary teaching of mathematics has been built on shaky foundations. Here is more solid ground, cleared of all the rubbish which prevents the ground being properly surveyed.

History of Air Navigation.

A. J. Hughes, O.B.E., F.R.Ae.S., F.R.G.S. George Allen & Unwin, Ltd., London 1946. 10/6 net.

The author for fifty years has been a maker and designer of navigation instruments, both marine and aircraft, and in the air was responsible for the Aperiodic compass and Bubble sextant, and at sea for the Echo sounder and Dead-beat compass. His tables for the air and sea are well known, as his firm has been for over 150 years.

With such a background it is not astonishing that the author has written a book of such interest and so well. He wrote most of the book during the worst period of the flying bombs as an " antidote to their scurrility and meanness . . . on this December night of 1944, with a waning moon and Orion in the south, I draw my faith and belief in the future of civil aviation from the navigators of the British Empire and Dominions, with its far-flung contacts and achievements."

Brave words to write at the time, but it is a brave heart that sings when the world is all awry.

It is remarkable when one reads of the early voyagers Bartholomew Diaz, Vasco da Gama, Columbus, Ferdinand Magellan and others, with what crude navigational aids they proved so successful, and to learn from the author that it was not until A.D 1400 that the science of navigation began to be studied seriously. The first Nautical Almanac was not

published until 1766, although books on navigation began to be published two hundred years earlier. In 1728 came the invention of the chronometer. The first one was made by John Harrison and is still in going order.

This is, however, largely a book on the development of air navigation, and there are many hitherto unpublished extracts from the logs of famous pilots on their great pioneering flights. The author was a personal friend of most of them and received first-hand details of many of their flights. He publishes Chichester's plotting of that amazing solo flight in a Moth on the 1st April, 1931, between Norfolk Island (8,000 acres) and Lord Howe Island (3,200 acres) in the Tasman Sea, a distance of 575 miles which took 7 hours 40 minutes. Chichester had 9½ hours petrol on board, with the knowledge that if he missed his objective he was certain to land in the sea.

As a contrast, on the 11th July, 1943, a Catalina flying-boat completed the first of a series of regular flights over 3,500 miles of ocean between Australia and Ceylon. What seamen took several hundred years to learn the airmen had to learn in not more than thirty.

Mr. Hughes quotes the tale of the Gremlins and one cannot refrain from quoting two verses which indicate when the Gremlins appear, when, indeed:—

" You're seven miles up in the heavens
(That's a hell of a lonely spot),
And its fifty degrees below zero,
Which isn't exactly hot;
When you're frozen blue like your Spitfires,
And you're scared a Mosquito pink;
When you're thousands of miles from nowhere,
And there's nothing below but the drink."

A book for all pilots and would-be pilots to buy, read, learn and inwardly digest.

Aircraft Engineer's " C " Licence.

Group Captain G. W. Williamson, M.Inst.C.E., M.I.Mech.E., F.R.Ae.S., George Newnes, Ltd., London, 1946. 17/6 net.

This is the second of a series of Aircraft Engineer's Licence books which Group Captain Williamson is writing to cover the work required for all five licences.

The author has kept up the high standard set in his first book on the "A" licence. There are twelve chapters covering duties, responsibilities and inspection methods, types of construction, propellers, engine controls and instruments, carburettors and ignition, fuel, oil and cooling systems, overhauls and testing afterwards, and study for the actual examination.

Each chapter is followed by a series of questions and books which are recommended for reading.

Few people are better qualified by experience and by the clarity of his writing than the author. This review may be short, but it is written on the principle that all the best things are packed in small parcels. This book is one which comes within that category and should be bought by all who are taking the " C " licence.

AN APPRECIATION

The Council of the Royal Aeronautical Society wish to thank those Companies who, by their generous co-operation, have done so much to help in the production of the Journal.

ACCLES & POLLOCK LTD.

A&P
Make it with Tubes

AIRCRAFT MATERIALS LIMITED

AML
STRUCTURAL MATERIALS and COMPONENTS

AERASPRAY MFG. CO., LTD.

AERASPRAY

AIRCRAFT PRODUCTION

AIRCRAFT PRODUCTION
AN ASSOCIATED ILIFFE PUBLICATION

"THE AEROPLANE"

THE AEROPLANE
A TEMPLE PRESS PUBLICATION

AIRSPEED LIMITED

AIRSPEED

AEROPLANE & MOTOR ALUMINIUM CASTINGS LTD.

ALUMINIUM CASTINGS

AIR TRANSPORT

AIR TRANSPORT
AND AIRPORT ENGINEERING
A TEMPLE PRESS PUBLICATION

AIRWORK LTD.

AIRWORK LIMITED

THE AUTOMATIC COIL WINDER & ELECTRICAL EQUIPMENT CO., LTD.

AVO ELECTRICAL TESTING INSTRUMENTS

ALVIS LIMITED

ALVIS

AUTOMOTIVE PRODUCTS COMPANY LTD.

HIGH PRECISION HYDRAULICS

Lockheed

ARMSTRONG SIDDELEY MOTORS LIMITED

ARMSTRONG SIDDELEY

AVIMO LTD.

AVIMO

OPTICAL - MECHANICAL - ELECTRICAL INSTRUMENTS
AERONAUTICAL EQUIPMENT

SIR W. G. ARMSTRONG WHITWORTH AIRCRAFT LTD.

SIR W. G.
ARMSTRONG WHITWORTH
AIRCRAFT LIMITED

BAKELITE LIMITED

TREFOIL
BAKELITE PLASTICS
REGD. TRADE MARKS

AUSTER AIRCRAFT LIMITED

THE STEEL AUSTER AEROPLANE
THE EYES OF THE ARMY

T. M. BIRKETT & SONS LTD.

BIRSO
TRADE MARK

NON-FERROUS CASTINGS
AND MACHINED PARTS
HANLEY - - STAFFS

BIRMINGHAM ALUMINIUM CASTING (1903) CO., LTD.

BIRMAL

THE BRITISH ALUMINIUM CO., LTD.

BA

BLACKBURN AIRCRAFT LTD.

Blackburn
AIRCRAFT

THE BRITISH AVIATION INSURANCE CO. LTD.

BA

BRITISH AVIATION SERVICES LTD.

JAMES BOOTH & COMPANY LIMITED

BOOTH
BIRMINGHAM
DURALUMIN
REGISTERED TRADE MARK

BRITISH INSULATED CALLENDER'S CABLES LTD

B.I.
Callender's

BOULTON PAUL AIRCRAFT LTD.

BPA

BRITISH PLASTICS

British Plastics
AN ASSOCIATED ILIFFE PUBLICATION

THE BRISTOL AEROPLANE CO., LTD.

"Bristol"

BRITISH ROPES LIMITED

BRITISH ROPES LIMITED
BR

HEAD OFFICE: DONCASTER

BRITISH THOMSON-HOUSTON CO., LIMITED (THE)

B T-H

Magnetos and Electrical Equipment

DAVID BROWN & SONS (HUDDERSFIELD) LTD.

David Brown

BRITISH WIRE PRODUCTS LTD.

everything under perfect control

aircraft controls

BUTLERS LIMITED

BUTLERS LIMITED BIRMINGHAM ATLANTIC BEAM THE SEAL OF QUALITY

AUTOMOBILE, AIRCRAFT AND MARITIME LAMPS
SEARCHLIGHTS AND MOTOR ACCESSORIES

THE BROOKE TOOL MANUFACTURING CO., LIMITED

DESIGNERS & MANUFACTURERS
OF
JIGS AND FIXTURES
AIRCRAFT COMPONENTS
ENGINEERS' SMALL TOOLS

BROOKE TOOL CARDINAL

THE CHLORIDE ELECTRICAL STORAGE CO. LTD.

Exide

AIRCRAFT BATTERIES

BROOM & WADE LTD.

"BROOMWADE"

COOPER & CO (B'HAM) LTD.

COOPERS

INDUSTRIAL FELT PRODUCTS

BROWN BROTHERS (AIRCRAFT) LTD.

Brown Brothers (AIRCRAFT) Ltd.

CONTROLS · AEROJACKS · A.C.S. & A.S. PARTS

THE DE HAVILLAND AIRCRAFT COMPANY LIMITED

DH

DELCO-REMY & HYATT LTD.

DELCO
DRH
ELECTRIC MOTORS

FAIREY AVIATION COMPANY LTD.

FAIREY
HIGH-PERFORMANCE SERVICE AIRCRAFT

DOWTY EQUIPMENT LIMITED

DOWTY
UNDERCARRIAGE AND HYDRAULIC EQUIPMENT

FERODO LIMITED

FERODO
REGD. TRADE MARK
BRAKE & CLUTCH LININGS

DUNFORD & ELLIOTT (SHEFFIELD) LTD.

Dunelt
Special steels
FOR AIRCRAFT

THOS. FIRTH & JOHN BROWN LTD.

TF JB
FIRTH BROWN
STEELS

DUNLOP RUBBER CO., LTD.

DUNLOP

FIRTH-VICKERS STAINLESS STEELS LTD.

"Staybrite"
FV

ENGLISH STEEL CORPORATION LTD.

STEEL MANUFACTURERS
ESC
ENGLISH STEEL CORPORATION LTD
SHEFFIELD

FLIGHT PUBLISHING CO., LTD.

FLIGHT
and
AIRCRAFT ENGINEER
AN ASSOCIATED ILIFFE PUBLICATION

FOLLAND AIRCRAFT LTD.

HANDLEY PAGE LTD.

GENERAL AIRCRAFT LTD.

HAWKER AIRCRAFT LTD.

GIRLING LIMITED

HELLIWELLS LTD.

GLOSTER AIRCRAFT CO., LTD.

THE HESTON AIRCRAFT CO., LTD.

GRAVINER MANUFACTURING COMPANY LTD.

HIGH DUTY ALLOYS LTD.

H. M. HOBSON (AIRCRAFT AND MOTOR) COMPONENTS LTD.

Hobson

IMPERIAL CHEMICAL INDUSTRIES LIMITED

ICI

F. A. HUGHES & CO., LIMITED

Distributors of

ELEKTRON

REGISTERED TRADE MARK

MAGNESIUM ALLOYS

INTEGRAL LTD.

INTEGRAL

HENRY HUGHES & SON LTD.

REGISTERED TRADE MARK HUSUN HENRY HUGHES & SON LTD LONDON

IRVING AIR CHUTE OF GREAT BRITAIN LTD.

AIR CHUTES IRVIN

THE HUGHES-JOHNSON STAMPINGS LTD.

JABLO PROPELLERS LIMITED

JABLO PROPELLER BLADES

HUNTING AVIATION LIMITED
(formerly Necaco Ltd.)

HUNTING

K.D.G. INSTRUMENTS LTD.

PRESSURE SENSITIVE K.D.G.

KELVIN BOTTOMLEY & BAIRD LTD.

" LIGHT METALS "

A TEMPLE PRESS PUBLICATION

K.L.G. SPARKING PLUGS LTD.

LODGE PLUGS LIMITED

ARTHUR LEE & SONS LTD.

THE LONDON NAME PLATE MANUFACTURING CO., LTD.

LEYTONSTONE JIG & TOOL CO., LTD.

MARSTON EXCELSIOR LIMITED

LIGHT-METAL FORGINGS LTD.

MILES AIRCRAFT LIMITED

MOLLART ENGINEERING COMPANY LTD.

NITRALLOY LIMITED

NITRALLOY LIMITED
NITROGEN HARDENING STEELS

THE MOND NICKEL COMPANY LTD.

NICKEL

NORMALAIR LTD.

MORRISONS ENGINEERING LTD.
MORRISON'S AIRCRAFT SERVICES

MORRISONS
CROYDON

THE PALMER TYRE LIMITED

'First in the Air'

Palmer

THE PALMER TYRE LIMITED
Herga House, Vincent Square, London, S.W.1.

D. NAPIER & SON LIMITED

PARNALL AIRCRAFT LIMITED

PARNALL AIRCRAFT
LIMITED

PERCIVAL AIRCRAFT LIMITED

PERCIVAL AIRCRAFT

" PLASTICS "

Plastics
A TEMPLE PRESS PUBLICATION

PETO & RADFORD

DAGENITE
AIRCRAFT BATTERIES

PORTSMOUTH AVIATION LIMITED

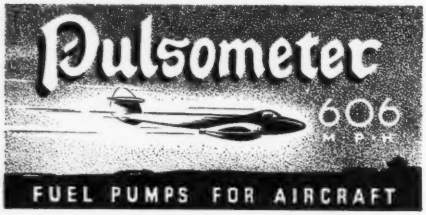
SIR ISAAC PITMAN & SONS LIMITED

Pitman
AERONAUTICAL
PUBLISHERS

PRESSED STEEL CO., LTD.

PRESTCOLD
Refrigeration

THE PULSOMETER ENGINEERING CO. LTD.



PYE LIMITED

PYE
RADIO . TELEVISION
TELECOMMUNICATIONS

QANTAS EMPIRE AIRWAYS

ROLLS-ROYCE LIMITED

ROLLS-ROYCE
AERO-ENGINES

REYNOLDS TUBE CO., LTD.

REYNOLDS TUBE CO. LTD.
REYNOLDS ROLLING MILLS LTD.
REYNOLDS
LIGHT ALLOY PRODUCTS
COLD DRAWN SEAMLESS STEEL TUBES
AND FABRICATED TUBULAR STRUCTURES

ROTAX LTD.

ROTAX
AIRCRAFT ELECTRICAL ENGINEERS

R.F.D. COMPANY LTD.

R·F·D
COMPANY LTD

SANGAMO WESTON LTD.

AIRCRAFT
Weston
INSTRUMENTS

A. V. ROE & COMPANY LIMITED

AVRo

SAUNDERS-ROE LTD.

SARO

SELF-PRIMING PUMP AND ENGINEERING CO., LTD.

SPECIALISTS in Aircraft Fuel Equipment

SKYHI LIMITED

"SKYHI"

SERCK RADIATORS LIMITED

SERCK

SMITHS AIRCRAFT INSTRUMENTS LTD.

SMITHS

SHORT BROTHERS (ROCHESTER & BEDFORD) LIMITED

Shorts

Established 1908

THE FIRST MANUFACTURERS OF AIRCRAFT IN THE WORLD

THE SPERRY GYROSCOPE COMPANY LTD.

SPERRY

SIMMONDS AEROCESSORIES LTD.

STANDARD TELEPHONES & CABLES LTD.

Standard Radio

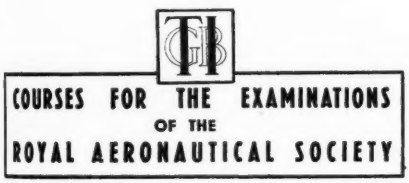
STRAIGHT AVIATION TRAINING LTD

THE UNITED STEEL COMPANIES LIMITED

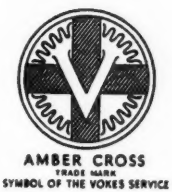
TECALEMIT LTD.

VICKERS-ARMSTRONGS LIMITED

THE TECHNOLOGICAL INSTITUTE OF GREAT BRITAIN



VOKES LTD



ULTRA ELECTRIC LTD.

WARWICK AVIATION CO. LTD.

WESTLAND AIRCRAFT LIMITED

WILKINSON RUBBER LINATEX LTD

FLEXATEX

WILKINSON PRODUCT

CHAS. WESTON & CO., LTD.

we have moved

From Coventry, Leamington Spa & Wootton Wawen

Head Office & Works

CHARLES

WESTON

& CO. LTD.

IRWELL BANK WORKS

DOUGLAS GREEN, MANCHESTER · 6

Phone: Pendleton 2857 8 9

Grams: Gitseals-Manchester

WILLIAMSON MANUFACTURING CO., LTD.

A. C. WICKMAN LIMITED

A.C. Wickman LIMITED

• COVENTRY • ENGLAND •

SPECIALISTS IN MACHINE TOOLS & SMALL TOOLS FOR THE AIRCRAFT INDUSTRY

YORKSHIRE ENGINEERING SUPPLIES LTD.

BRONZE FOUNDRIES

YESEATONIA

BEARINGS AND BUSHES

HENRY WIGGIN & COMPANY LTD

For High Quality

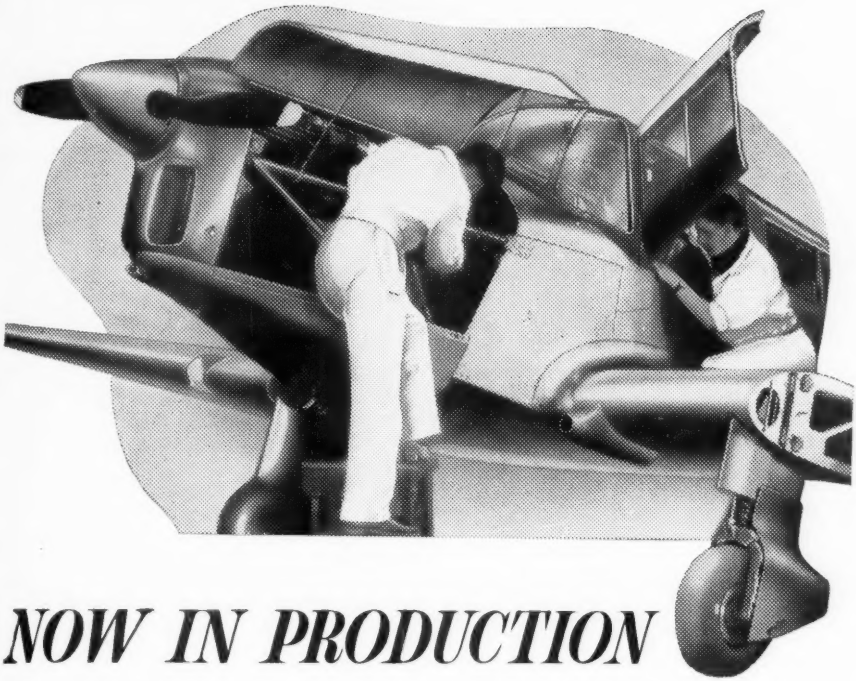
NICKEL ALLOYS

DIRECTORY OF ADVERTISERS

Accles & Pollock Ltd.	Paddock Works, Oldbury, Birmingham	Broadwell 1500
Aerospray Mfg. Co. Ltd.	Thimble Mill Lane, Birmingham, 7	Birmingham E. 1671
"Aeroplane, The"	Temple Press Ltd., Bowling Green Lane, E.C.1	Terminus 3636
Aeroplane & Motor Aluminium Castings Ltd.	Wood Lane, Erdington, Birmingham	Erdington 2207-9
Aircraft Production	Dorset House, Stamford Street, S.E.1	Waterloo 3333
Aircraft Materials Ltd.	Midland Road, London, N.W.1	Euston 6151
Airspeed Ltd.	The Airport, Portsmouth	Portsmouth 74631
Air Transport	Temple Press Ltd., Bowling Green Lane, E.C.1	Terminus 3636
Airwork Ltd.	Westbrook House, 134 Bath Road, Hounslow	Hounslow 5451
Alvis Ltd.	Holyhead Road, Coventry	Coventry 5501
Armstrong Siddeley Motors Ltd.	Parkside, Coventry	Coventry 4061
Armstrong Whitworth Aircraft, Sir W. G., Ltd.	Whitley, Coventry	Coventry 61061-4
Auster Aircraft Ltd.	Britannia Works, Thurmaston, Leicester	Syston 86106-8
Automatic Coil Winder & Electrical Equipment Co. Ltd.	Winder House, Douglas Street, S.W.1	Victoria 3404-8
Automotive Products Co. Ltd.	Tachbrook Road, Leamington Spa	Leamington Spa 1700
Avimo Ltd.	Taunton, Somerset	Taunton 3634
Bakelite Ltd.	18 Grosvenor Gardens, S.W.1	Sloane 9911
Birkett, T. M., & Sons Ltd.	Thomas Street, Hanley, Stoke-on-Trent	Stoke-on-Trent 2184
Birmingham Aluminium Casting (1903) Co. Ltd.	Dartmouth Road, Smethwick, 40, Staffs.	Smethwick 1431
Blackburn Aircraft Ltd.	Brough, E. Yorks	Brough 121
Booth, James, & Co. Ltd.	Argyle Street Works, Nechells, Birmingham, 7 ...	Birmingham E. 1221
Boulton Paul Aircraft Ltd.	Wolverhampton, Staffs	Fordhouses 3191
Bristol Aeroplane Co. Ltd., The	Filton House, Bristol	
British Aluminium Co. Ltd.	Salisbury House, London Wall, E.C.2	Clerkenwell 3494
British Aviation Insurance Co. Ltd.		
British Aviation Services Ltd.	3-4 Lime Street, E.C.3	Mansion House 0444
British Insulated Callender Cables Ltd. ...	Main Works: Erith, Helsby, Leigh, Lancs. and Prescot	
British Plastics	Dorset House, Stamford Street, S.E.1	Waterloo 3333
British Ropes Ltd.	52 High Holborn, W.C.1	Chancery 8822
British Thomson-Houston Co. Ltd.	Lower Ford Street, Coventry	Coventry 4104
British Wire Products Ltd.	Worcester Road, Stourport-on-Severn	Stourport 240
Brooke Tool Manufacturing Co. Ltd. ...	Warwick Road, Greet, Birmingham, 11	B'ham Victoria 2323
Broom & Wade Ltd.	High Wycombe, Bucks.	High Wycombe 1630
Brown Brothers (Aircraft) Ltd.	Great Eastern Street, E.C.2	Bishopsgate 7654
Brown, David, & Sons (Huddersfield) Ltd.	Huddersfield	Huddersfield 3500
Butlers Ltd.	Atlantic Works, Grange Road, Small Heath, B'ham, 10	Victoria 2164-6
Chloride Electrical Storage Co. Ltd., The	Exide Works, Clifton Junction, near Manchester ...	Swinton 2011
Cooper & Co. (B'ham) Ltd.	53 Little King Street, Birmingham, 19	Northern 2194-5
de Havilland Aircraft Co. Ltd., The ...	Hatfield Aerodrome, Herts.	Hatfield 2345
Delco-Remy & Hyatt Ltd.	111 Grosvenor Road, S.W.1	Victoria 6242
Dowty Equipment Ltd.	Cheltenham, Glos.	Cheltenham 53471
Dunford & Elliott (Sheffield) Ltd.	Attercliffe Wharf Works, Sheffield, 9	Sheffield 41121-4
Dunlop Rubber Co. Ltd.	Fort Dunlop, Erdington, Birmingham, 24	Erdington 2121
English Steel Corporation Ltd.	Vickers Works, Sheffield	Sheffield 41071
Fairey Aviation Co. Ltd.	Hayes, Middlesex	HAYes 1800
Ferodo Ltd.	Chapel-en-le-Frith, Stockport	Chapel-en-le-Frith 250
Firth, Thos., & John Brown Ltd.	Atlas & Norfolk Works, Sheffield, 1	Sheffield 20081, 26491
Firth-Vickers Stainless Steels Ltd. ...	Staybrite Works, Sheffield	Sheffield 41193
Flight Publishing Co. Ltd.	Dorset House, Stamford Street, S.E.1	Waterloo 3333
Folland Aircraft Ltd.	Hamble, Southampton, Hants.	Hamble 3191
General Aircraft Ltd.	The London Air Park, Feltham, Middlesex	Feltham 3636
Girling Limited	Garrison Lane, Birmingham, 9	Victoria 2951
Gloster Aircraft Co. Ltd.	Gloster Works and Aerodrome, Hucclecote, Glos. ...	Gloucester 6294
Graviner Manufacturing Co. Ltd.	53 Pall Mall, S.W.1.	Whitehall 6478-9
	Graviner Works, Gosport Road, near Fareham, Hants.	Gosport 89175-7
Handley Page Ltd.	Cricklewood, N.W.2	Gladstone 8000
Hawker Aircraft Ltd.	Canbury Park Road, Kingston-on-Thames	Kingston 1044
Hellwells Ltd.	Old Town, Stratford-on-Avon	Slough 23861 Stratford-on-Avon 3265
Heston Aircraft Co. Ltd., The	Heston Airport, Middlesex	Southall 2321
High Duty Alloys Ltd.	89 Buckingham Avenue, Slough, Bucks.	Slough 21201
Hobson, H. M. (Aircraft & Motor) Components Ltd.	Hobson Works, Stafford Road, Wolverhampton	Fordhouses 2266
Hughes, F. A., & Co. Ltd.	Metals Dept., Abbey House, Baker Street, N.W.1 ...	Welbeck 2332-6
Hughes, Henry, & Son Ltd.	Husun Works, New North Road, Barkingside, Essex ...	Hainault 2601
Hughes-Johnson Stampings Ltd., The ...	Langley Green, Birmingham	Broadwell 1361
Hunting Aviation Ltd.	P.O. Box 4 Caernarvon, N. Wales	Caernarvon 580
Imperial Chemical Industries Ltd.	London, S.W.1	Victoria 4444

DIRECTORY OF ADVERTISERS

INTEGRAL LTD.	Cousins Street, Dudley Road, Wolverhampton	Wolverhampton 24984-6
IRVING AIR CHUTE OF GREAT BRITAIN LTD.	Ickneild Way, Letchworth, Herts.	Letchworth 888
JABLO PROPELLERS LTD.	Mill Lane, Waddon, Croydon, Surrey	Croydon 2201
K.D.G. INSTRUMENTS LTD.	Purley Way, Croydon, Surrey	Thornton Heath 3866
KELVIN, BOTTOMLEY & BAIRD LTD.	Kelvin Avenue, Hillington, Glasgow, S.W.2	Halfway 3331
	Kelvin Works, Basingstoke	Basingstoke 690
K.L.G. SPARKING PLUGS LTD.	Putney Vale, S.W.15	Putney 2671
LEE, ARTHUR, & SONS LTD.	Crown Steel & Wire Mills, Bessemer Road, Sheffield, 9	Attercliffe 41144-8
	Mowbray House, Norfolk Street, W.C.2	Temple Bar 7187-8
	155 Church Lane, Handsworth Road, Birmingham, 20	Northern 2116-7
LEYTONSTONE JIG & TOOL CO. LTD.	606 High Road, Leyton, E.10	Leytonstone 5022-3
LIGHT-METAL FORGINGS LTD.	Oldbury, Birmingham	Broadwell 1152
LIGHT METALS	Temple Press Ltd., Bowling Green Lane, E.C.1 ...	Terminus 3636
LODGE PLUGS LTD.	Rugby	Rugby 2076
LONDON NAME PLATE MANUFACTURING CO. LTD., THE	Zylo Works, Marine View, Brighton, 7	Brighton 7025
MARSTON EXCELSIOR LTD.	Wolverhampton	Wolverhampton 21481
	49-59 Armley Road, Leeds, 12	Armley 38081-5
MILES AIRCRAFT LIMITED	Reading, Berkshire	Reading 60811
MOLLART ENGINEERING CO. LTD.	Kingston By-Pass, Surbiton, Surrey	Elmbridge 3352-4
MOND NICKEL CO. LTD., THE	Grosvenor House, Park Lane, W.1	Grosvenor 4131
MORRISONS ENGINEERING LTD.	Purley Way, Croydon	Croydon 0191
NAPIER, D., & SON LTD.	Acton, W.3	Shepherds Bush 1220
NITRALLOY LTD.	47, Bank Street, Sheffield	Sheffield 25907
NORMALAIR LTD.	West Hendford, Yeovil, Somerset	Yeovil 1100
PALMER TYRE LTD., THE	Herga House, Vincent Square, S.W.1	Victoria 8323
PARNALL AIRCRAFT LTD.	8 South Street, Park Lane, W.1	Grosvenor 4491
PERCIVAL AIRCRAFT LTD.	Luton Airport, Luton, Beds.	Luton 2960
PETO & RADFORD	Chequers Lane, Dagenham, Essex	Rainham 34
PITMAN, SIR ISAAC, & SONS LTD.	Parker Street, Kingsway, W.C.2	Holborn 9791
PLASTICS	Temple Press, Bowling Green Lane, E.C.1	Terminus 3636
PORTSMOUTH AVIATION LTD.	The Airport, Portsmouth	Portsmouth 74374
PRESSED STEEL CO. LTD.	Cowley, Oxford	Oxford 77701
PULSOMETER ENGINEERING CO. LTD., THE ...	Nine Elms Iron Works, Reading; 39 Victoria St., S.W.1	Tilehurst 67182/4
PYE LIMITED	Radio Works, Cambridge	Cambridge 3434
QANTAS EMPIRE AIRWAYS	c/o British Overseas Airways Corp., Airways Terminal, Buckingham Palace Road, S.W.1	Victoria 3126
REYNOLDS TUBE CO. LTD.	Hay Hall Works, Tyseley, Birmingham	Acocks Green 1607
R.F.D. COMPANY LTD.	Stoke Road, Guildford, Surrey	Guildford 3232
ROE, A. V., & CO. LTD.	Newton Heath, Manchester	Failsworth 2020
ROLLS-ROYCE LTD.	Derby	Derby 2424
ROTAX LTD.	Willesden Junction, N.W.10	Elgar 7777
SANGAMO WESTON LTD.	Great Cambridge Road, Enfield, Middlesex	Enfield 3434 & 1242
SAUNDERS-ROE LTD.	49 Parliament Street, Westminster, S.W.1	Whitehall 7271
SELF-PRIMING PUMP & ENGINEERING CO. LTD.	Edinburgh Avenue, Trading Estate, Slough, Bucks. ...	Slough 23277
SERCK RADIATORS LTD.	Warwick Road, Birmingham, 11	Victoria 0531
SHORT BROTHERS (ROCHESTER & BEDFORD) LTD.	Rochester, Kent	Chatham 2261
SIMMONDS AEROCESSORIES LTD.	Great West Road, Brentford, London	Ealing 2212
SKYHI LTD.	"Skyhi" Works, Worton Road, Isleworth, Middlesex	Hounslow 2211
SMITHS AIRCRAFT INSTRUMENTS LTD.	Cricklewood Works, London, N.W.3	Gladstone 3333
SPERRY GYROSCOPE CO. LTD., THE	Great West Road, Brentford, Middlesex	Ealing 6771
STANDARD TELEPHONES & CABLES LTD. ...	New Southgate, N.11	Enterprise 1234
STRAIGHT AVIATION TRAINING LTD.	Bush House, Aldwych, W.C.2.	Temple Bar 6828
TECALEMIT LTD.	Great West Road, Brentford, Middlesex	Ealing 6661
TECHNOLOGICAL INSTITUTE OF GREAT BRITAIN LTD., THE	39 Temple Bar House, Fleet Street, E.C.4	Central 5940
ULTRA ELECTRIC LTD. AIRCRAFT DIVISION ...	Western Avenue, Acton, W.3	Acorn 3434
UNITED STEEL COMPANIES LTD., THE	17 Westbourne Road, Sheffield, 10	Sheffield 60081
VICKERS - ARMSTRONGS LIMITED (AIRCRAFT SECTION)	Vickers House, Broadway, Westminster, S.W.1	Abbey 7777
	Weybridge Works, Weybridge, Surrey	Byfleet 240-243
VOKES LTD.	Henley Park, nr. Guildford, Surrey	Guildford 62861
WARWICK AVIATION CO. LTD.	Warwick	Warwick 693
WESTLAND AIRCRAFT LTD.	Yeovil	Yeovil 1100
WESTON, CHAS., & CO. LTD.	Torrington Avenue, Coventry	Tile Hill 66291-2
WICKMAN, A. C., LTD.	Tile Hill, Coventry	Tile Hill 66271
WIGGIN, HENRY, & CO. LTD.	Wiggin Street, Birmingham	Edgbaston 2245
WILKINSON RUBBER LINATEX LTD.	Frimley Road, Camberley, Surrey	Camberley 1595
WILLIAMSON MANUFACTURING CO. LTD. ...	Litchfield Gardens, Willesden Green, N.W.10	Willesden 0073-0075
YORKSHIRE ENGINEERING SUPPLIES LTD. ..	Bronze Foundries, Upper Wortley Road, Leeds, 12 ...	Leeds 38234 & 38291



NOW IN PRODUCTION

● **TAXI SERVICE.** *The Proctor's robust construction, steadiness in all conditions and reliability make it ideal for taxi-service. Four comfortable seats with ample luggage space and a cruising speed of 140 miles per hour.*

The War service of the Proctor has enhanced its already high reputation as an aristocrat among light aircraft. The civil model now in production is remarkable for these outstanding features: Stability and Ease of Control; Accessibility and Ease of Service; Generous Equipment, including full blind and night flying instruments, landing lamps, navigation, cabin and instrument lights; provision for radio (engine fully screened and airframe bonded). Fitted with the de Havilland Gipsy Queen II engine with constant speed propeller. Inclusive Price, ex works, **£3,300**. Delivery three months from order.

● **FLYING CLUBS** *will flourish or fail according to their accident rate. The Proctor is an ideally safe machine; lively but easy and responsive.*

● **CHARTER SERVICE.** *Business Executives travelling by air will want speed, comfort and safety. Seats may be removed and light freight carried.*

● **PRIVATE OWNER.** *A lively modern aircraft with 'breeding'. That's what the private owner wants. Plus, of course, reliability, comfort and accessibility for service. He'll find all he expects in the Proctor—the aristocrat of light aircraft.*

THE PERCIVAL PROCTOR

is made for those jobs.

LUTON AIRPORT, BEDFORDSHIRE, ENGLAND.

A COMPANY OF THE HUNTING GROUP.

C.R.C.9

DUNLOP
HIGH PRESSURE HOSE ASSEMBLY

Approved for working pressures up to 3,000 lb. sq. in. Suitable for Air, Hydraulic Fluids, Engine Oil, Petrol, Glycol, etc.

DUNLOP RUBBER Co., Ltd., *Aviation Division*, COVENTRY

6H/909

Printed by the Lewes Press (Wightman & Co. Ltd.), Friars Walk, Lewes, England, and Published by the Royal Aeronautical Society, 4, Hamilton Place, London, W.1, England.

R Y

Royal